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I. THE SATURNIAN SYSTEM.

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IT has always appeared to me, since first I studied the subject of Saturn and his system, that our books on astronomy fail to indicate effectively the position which this noble planet, and the scheme over which he holds sway, bear in the economy of the solar system. The remark extends to Jupiter, and in a less degree to Uranus and Neptune. There is to my mind something most incongruous between the true teachings of astronomy respecting the giant planets, and the notions complacently presented in book after book on elementary astronomy, and even in treatises by masters of the subject. We have the astronomy of the ancients and the modern astronomy intermixed. We see rightly taught the dimensions and general aspect of the different planets, but we find these bodies classed together precisely as they very reasonably were when astronomers knew little more than that there are, besides the sun and moon, the planets Mercury, Venus, Mars, Saturn, and Jupiter. The orbits of these bodies are plotted down in a series of concentric and equidistant circles, on which very commonly are shown pictures (save the mark) of the several planets, and the reader is left to combine the utterly erroneous notions thus indicated with such ideas as he may derive from the array of numbers contained in the tables of planetary elements. Nor in the verbal description of the planets is any stress laid upon the characteristic peculiarities which distinguish the outer family of planets from the inner. Differences are stated, but mere statement in such cases counts for very little; the impression really conveyed is, that whereas the earth, and Mars, and Venus, and Mercury are so many smaller worlds, Jupiter, and Saturn, and Uranus, and Neptune are as many larger worlds; and whatever peculiarities distinguish these outer and larger planets from the rest are discussed with direct reference to familiar terrestrial relations. For instance, if the satellites of Saturn are referred to, the remark is made that the external skies of Saturn must be well illuminated, and very beautiful with all these moons, when our own single moon forms so beautiful a feature of our nights. When the rapid rotation of Saturn is mentioned, we are told immediately that therefore the day in Saturn lasts only so many of our hours. So with the Saturnian year, because the mean density of Saturn is but about 13-100ths of the earth's; we are told that, therefore, his globe is constructed of material as light as mahogany; and so on, through a variety of such comparisons.

It appears to me that the history of investigations into the physical condition of the planets is characterised by a very singular unreadiness to view the whole subject from the new standpoint, available when telescopic observations began to be made. The new knowledge gained by means of the telescope was welded to the old ideas respecting the planets. New cloth was added to old garments. The natural course, one would have supposed, would have been to consider the planets altogether in the light of information obtained by the telescope, simply because that was the first really reliable information obtained by astronomers. Everything until then had been guess work; yet the results of such guess work still appears in our books on astronomy. It is not Saturn or Jupiter, as revealed by the telescope, with which our writers on astronomy deal; but they tell us, in effect, that the Saturn and Jupiter of the old astronomers have been found to have such and such dimensions, rotation-rates, aspect, and so on.

Accordingly, the attempt to reason respecting these planets in perfect independence of the ideas entertained ages ago by astronomers, is regarded as a species of innovation. That is held to be rash and fanciful speculation which in point of fact is the only scientific way of treating the subject. It is held to be a sort of heresy to speak of Saturn or Jupiter in terms not strictly compatible with the words of Sir W. Herschel, for example, although his ideas respecting these planets were based entirely on the ideas formerly entertained about them, and conveyed to Sir W. Herschel through the instructive but not very suggestive teachings of Ferguson. I have not, indeed, myself had to complain on this point. I have, in fact, been surprised at the exceedingly liberal manner in which my own theoretical opinions have been received—I may even say welcomed—by many who, nevertheless, still retain a prejudice for the older way of treating the subject. But I observe that other writers have been less fortunate; and when they have indicated the significance of the features which distinguish giant planets from the terrestrial planets, and touch on some of the conclusions flowing from such considerations, their views are scouted as wild and fanciful—too startling, in fact, to obtain acceptance among men of science.

In passing, I would touch on the objection to startling views, merely because they are startling, as utterly unreasonable in the presence of all that has been discovered, not merely in astronomy, but in science generally. The accepted truths of science are nearly always startlingly unlike what has been imagined before the evidence became strong enough, or was well studied enough, to indicate the real facts. Take, for instance, what has been learned recently about the sun. Suppose that twenty years ago the now known truths about the constitution of the sun, about coloured prominences, and about the corona had been simply enumerated, either as the result of theoretical considerations or of observations the nature of which was not made known: then it is certain that the surprising character of these new views would have rendered them unwelcome to astronomers. Less startling theories would have been received with favour, and quoted as altogether preferable to notions so bizarre and fanciful; yet the sober theories of twenty years ago were wide of the truth. They were in reality fanciful, and though not far fetched, yet ill fetched; drawn, in fact, from utterly incorrect analogies.

It may be assumed, indeed, ordinarily, that the relations as yet unknown are such as we should regard as surprising. It is not merely an unsafe, but an almost certainly misleading assumption that the best explanation of facts is the one which is most obvious and natural. What can be more natural, for instance, than the theory that the solar corona is due to the passage of the sun's rays through our own atmosphere?--what more utterly misleading? How natural was Faye's theory that the solar sierra is a phenomenon produced by the lunar atmosphere; and yet the theory was altogether and demonstrably unsound. To go farther back in the history of astronomy, the Ptolemaic theory was unquestionably intended to explain in the most natural way the celestial motions observed from our earth, obviously fixed, obviously far larger than any of the heavenly orbs. Even more natural and seemingly obvious is the theory that the earth is a great plain. The true theory in these and hundreds of other instances has not been the theory which commended itself by its obviousness and by its close accordance with what was mistakenly regarded as the *natural* order of things.

I propose in the present essay on the Saturnian system to begin with the consideration of known facts about Saturn and his system, and from them to endeavour to educe just ideas respecting the constitution of the Saturnian system, proceeding in perfect independence of all preconceived opinions. I shall neither endeavour to support nor to overthrow the common notion that Saturn is a globe resembling our earth in nature, though larger and in certain details unlike the earth. I shall endeavour as far as possible to treat my subject as though the earth were not the sole orb in the universe with which we have a close acquaintance.

In the first place, then, let us consider Saturn's position in the solar system, as respects mass, the most important element of a planet's condition, simply because the mass of a planet measures the planet's power.

Regarding the solar system as a whole, we see the sun so largely surpassing all the planets together in mass and volume, that if we knew nothing else respecting him, we should recognise the fact that he belongs to another order of created things. He does not surpass the several planets only, but all the planets taken together.

But we are apt to overlook the fact that Jupiter and Saturn are as markedly distinguished from the earth and Venus as the sun is from the giant planets. Jupiter alone surpasses all the members of the inner or terrestrial family of planets, taken together, about a hundred and forty times. Saturn surpasses them all, taken together, about forty times. A difference such as this can hardly be described as one of degree merely. It is a difference of kind, so far as mass *can* indicate such a difference. It seems a sufficient proof of this to note that if the sun were destroyed as well as Saturn, Uranus, and Neptune, then Jupiter could effectively replace the sun as a ruling orb, round which the terrestrial families could travel, not indeed on such wide orbits as at present, but on paths of great extent, Jupiter remaining as appreciably stable at the centre of the scheme as the sun now is. The like can be said about Saturn, since his mass exceeds that of the earth—the largest member of the family of minor planets—about ninety times.

Even taking into account Uranus and Neptune, Jupiter and Saturn are supreme among the members of the solar system. Jupiter is indeed easily first, seeing that he surpasses about 2½ times (in Mars) all the other members of the family, including Saturn, taken together. But Saturn is as easily second, seeing that he in turn surpasses all the other members of the planetary scheme, taken together, nearly three times.

Still it is, when we compare either Jupiter or Saturn with the members of the terrestrial family of planets, or rather with that family as a whole, that we perceive most clearly that those giant planets belong to another order of bodies. Our earth does not to the same degree surpass the family of asteroids regarded as a whole. It is indeed rather surprising that the asteroids should so readily have been recognised as belonging to a distinct order, while the giant planets, which differ fully as much from the terrestrial family of planets as these planets differ from the asteroids, should be regarded as though they were members of one and the same family.

It may be urged, perhaps, that the asteroids by travelling altogether within a comparatively small region of the sun's domain seem marked out as forming a distinct family. But in truth, the region within which the asteroids pursue their path is very much larger than that occupied by the members of the terrestrial family of planets—certainly twice as large, if we leave altogether out of account the great range of the asteroids in distance from the mean plane of the solar system. The orbital range of the terrestrial family of planets is indeed so small, compared with that of the outer family, that the two sets of orbits cannot be properly represented in the same diagram. If a diagram includes the orbits of the giant planets properly drawn to scale, then the path of Mars, the outermost of the terrestrial planets, is represented by a circle considerably smaller than that commonly used to represent the sun in pictures of the solar system.

Let us endeavour, then, to picture to ourselves the giant globe of Saturn pursuing its career so far from the central orb that the region girt round by its orbit exceeds more than ninety-fold the region which the earth circuits in her annual revolution. Let us remember that in precisely the same degree the solar light and heat are reduced at Saturn's distance, and (also in the same degree) the attractive influence of the sun, and singularly enough, in the same degree, Saturn's own attractive influence exceeds the earth's. For the former relation was a necessary consequence of the law according to which light, heat, and gravity diminish with distance;* but the latter is, as it were, accidental, since there is no necessary connection between the mass of any planet and the distance at which it travels. It is worthy of notice, as a convenient aid to the memory, that while the area swept out by Saturn is about ninety times that swept out by the earth, and solar heat, light, and gravity at Saturn about one-ninetieth less than at the earth, the mass of Saturn exceeds that of the earth about ninety-fold. It is manifest, then, that Saturn must be regarded as a much more efficient ruler over the region of space along which he circles than the earth can possibly be over the region of space through which she travels. Or rather, it is manifest that the range of Saturn's influence is much wider. So feeble is the earth, so narrow is the sphere of her special influence, that even her own moon is far more fully under solar influence than under terrestrial rule. In fact, if we remember that the sun's mass is about 315,000 times that of the earth, we see that his influence equals hers at any point whose distance from the sun is to that from the earth as the square root of 315,000 to unity, or about as 561 to 1. Now since the distance of the earth from the sun is about 91,500,000 miles, a point on the line joining the earth and sun (and between these bodies) must be distant from the earth 1-562nd part of this distance to be equally influenced by the sun and earth. This distance is rather less than 163,000 miles, whereas the moon's distance from the earth amounts to 238,800 miles. A point beyond the earth on the line joining the sun and earth produced should be at 1-560th part of 91,500,000 miles from the earth, to be equally influenced by the earth and sun, that is about 164,000 miles. This is the farthest point from the earth at which her influence equals that of the sun; and if a sphere be described in space at any instant, having the line joining this last-named point and the one before determined (163,000 miles from the earth's centre *towards* the sun) as diameter, then at all points within this sphere with its diameter of 327,000 miles or thereabouts, the earth acts more potently than the sun; but everywhere else she acts less potently. Now let us apply a similar process to Saturn, in order to ascertain the dimensions of the sphere over which his power is, as it were, supreme, and we shall at once perceive how different is his position as a ruler of matter compared with that of the earth. The mean distance of Saturn from the sun amounts to 872,137,000 miles, and his mass is about 90 times the earth's, or about 1-3500th of the sun's mass. Hence the sun's influence equals Saturn's at any point whose distance from the sun is to that from Saturn as the square root of 3500 to unity, or about as 59 to 1. Hence a point on the line joining Saturn and the sun, and between these bodies, must be distant from Saturn 1-60th part of the distance of Saturn from the sun to be equally influenced by the sun and Saturn. This distance is about 14,500,000 miles from Saturn on the side towards the sun. On the farther side, a point equally influenced by Saturn and the sun would lie at a distance from Saturn equal to 1-58th part of Saturn's distance from the sun, or at a distance of about 15,000,000 miles. Hence the sphere over which Saturn bears supreme sway has a diameter of 29,500,000 miles. We have seen that the sphere over which the earth bears supreme sway has a diameter of only 327,000 miles. Hence Saturn's sphere of rule is to the earth's, in diameter, as 29,500 to 327, or is about 90 times greater.* Here strangely enough the proportion 90 to 1 comes in yet again, not as the reader might imagine at a first view, as a necessary consequence of the same proportion 90 to 1 in the mass of Saturn compared to the earth's, and in the square of his mean distance to the square of the earth's, but independently—since it would not have appeared as the result of our calculations did not the sun's mass bear to Saturn's the proportion 3500 to 1. The *volume* of the sphere ruled over by Saturn bears to the volume of the sphere ruled over by the earth a proportion of about 730,000 to unity; and it may not unfairly be said that this proportion indicates the relative position of the two orbs—Saturn and the earth—as respects power in the scheme of the planets. This proportion seems certainly more truly to indicate their relative power than the direct proportion between their masses—since we must recognise in the earth's relative proximity to the sun a source of comparative inferiority as a ruler over matter. It is noteworthy, moreover, that if we adopt this criterion, Saturn and Jupiter are brought almost to equality, notwithstanding the greatly superior mass of the last named planet.

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* Since the area swept out by a planet in completing its orbit varies directly as the square of the mean distance, while light, heat, and gravity vary inversely as the square of the mean distance, it follows that the light, heat, and attraction influencing bodies revolving around the same central sun vary inversely as the area of their orbits.

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* It may be interesting to determine in the same way the extent of the sphere over which Jupiter bears sway. His mean distance from the sun amounts to 475,692,000 miles, and his mass is equal to about one $\frac{1}{1048}$th part of the sun's. Hence the influence of Jupiter and the sun are equal at any point, whose distance from Jupiter is to its distance from the sun as 1 to the square root of 1048, or as 1 to about 32. Hence we must take from Jupiter on the side *towards* the sun a distance equal to $\frac{1}{33}$rd part of Jupiter's distance, and on the side away from the sun a distance equal to $\frac{1}{31}$st part of Jupiter's distance. These distances are respectively about 14,400,000 miles, and about 15,300,000 miles; so that the sphere over which Jupiter bears superior sway has a diameter of about 29,700,000 miles, which may be regarded as about equal to the diameter of the sphere over which Saturn bears superior sway, I have spoken of the region over which a planet bears special sway as a sphere, and this is actually the case. It is manifest from the reasoning that the planet is not centrally placed within the sphere, but is nearer the side towards the sun. The sphere is manifestly not constant in dimensions, being larger or smaller, according as the planet is farther from or nearer to the sun.

It is to be noticed that the relation here considered bears very importantly on the question of the original formation of the planetary system. I must admit that, for my own part, I find much in Laplace's conception of the genesis of the solar system, which is very far from satisfactory. A gradually contracting nebulous mass, such as he pictures, could scarcely in my opinion have produced a system in which the masses are at a first view so irregularly scattered as in the solar system. But if we adopt such a view as I have endeavoured to maintain in my "Other Worlds," we find in the position of the various members of the solar system a satisfactory reason for their various dimensions. According to that view the solar system had its origin in the gathering together of matter towards a great centre of aggregation. The subsidiary centres of aggregation which would as naturally arise during such a process as subsidiary whorls in a gigantic whirlpool, might be expected to have such dimensions as we actually observe. Close to the great centre, such centres would be relatively small, because the ruling centre would be supreme everywhere within the sphere close around him, except quite close to subordinate aggregations. No matter except such as passed very close to such aggregations would be gathered in. We may suppose that such aggregations would indeed only form on account of the enormous wealth of matter within that sphere, and the consequent certainty of collisions, or very close approaches resulting in agglomeration; and towards the outskirts of this part of the sphere of the sun's influence there would not even be any definite agglomeration, but a number of very small—and, as it were, accidental—aggregations resulting in the ring of asteroids. Outside the sphere of the sun's overmastering influence, there would still be a great wealth of matter, and gathering aggregations would exert their attractive energies far more widely. Nearest of all to the central region would come the mightiest aggregation of all, on account of the greater quantity of matter. Here, then, Jupiter had his birth, chief giant of the solar system, and prince of all the planets. But the range of sway would widen with increasing distance, and at first such widening would go far to counterbalance the gradual falling off in the gravity of matter. Moreover, with increase of distance from the sun would come a greater freedom to form out of the aggregating matter subordinate schemes or systems. Next, then, beyond Jupiter, with his giant bulk and uniform system of secondary bodies, came Saturn into being, inferior in mass to his giant brother, but ruling over a wider space, separated from Jupiter by a far greater distance than separates Jupiter from the asteroids, and in fact, according to the criterion I have laid down above, nearly the equal of Jupiter in the range of his independent power. Here we see the origin of the most remarkable system within the solar domain—a scheme of rings whose complicated structure is as yet not half understood, and a family of eight satellites as diverse in size and arrangement as the eight primary members of the solar family itself; and we can readily understand how, with yet increasing distances and continually decreasing quantity of matter, the minor but still gigantic masses of Uranus and Neptune should appear on the outskirts of the solar system. I venture to predict, with some degree of confidence, that if any trans-Neptunian planet be ever discovered, it will be inferior, but not greatly inferior, to both Uranus and Neptune in mass.*

Let us now, however, turn from the consideration of the mass and weight of Saturn, and of the processes by which he may be thought to have reached his present condition, to the inquiry what that condition probably is.

We see Saturn, then, the centre of a scheme of wonderful extent and importance. The outermost satellite circuits in an orbit having a diameter of more than 4,600,000 miles. This is the widest span of any satellite orbit, and not far short of ten times the span of our moon's orbit. The widest span of the Jovian satellite system amounts to but 2,380,000 miles, or almost exactly five times the span of the moon's orbit. It will be observed that both the Saturnian and

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* In old times this would have been a tolerably safe prediction, since any such planet would have been very unlikely indeed to be discovered. But in these days, when the zodiac is continually being swept for the discovery of new planets, and when new planets are being detected at an average rate of about five per annum, it is exceedingly unlikely that any trans-Neptunian planet will long escape detection, supposing any such planet exist to be detected.

Jovian satellite systems lie far within the limits of the spheres ruled over with superior sway by these planets respectively. For it will be remembered that Saturn's sphere of sway has a diameter of about 29,500,000 miles, while Jupiter's (see note at p. 7) is about 200,000 miles wider. The effect of this is seen in the motions of the Jovian and Saturnian satellite systems. For whereas the orbit of our moon around the sun is everywhere concave towards the sun, notwithstanding the motion of the moon round the earth, the paths even of the outermost of the Jovian and Saturnian satellites are markedly convex towards the sun when these bodies are in inferior conjunction. The inner satellites, in fact, not only move at those times on courses convex towards the sun, but have an excess of motion in their orbit round their primary over the onward motion which they share with him around the sun, and consequently travel backwards in this part of their motion. It follows that in successive lunations the inner satellites trace out looped paths. This happens with the four satellites Mimas, Euceladus, Tethys, and Dione. The fifth, Rhea, shows the singular peculiarity of coming almost exactly to rest when in inferior conjunction—that is, to rest relatively to the solar system. The path of this satellite with reference to the solar system is nearly an epicycloid, any portion of which, from cusp to cusp, is appreciably a cycloid. The satellites Titan, Hyperion, and Japetus follow waved courses convex to the sun through a considerable portion of each lunation. It will be seen that Saturn has his system in complete control.* Even the outermost is very much less perturbed by the sun (relatively as well as absolutely) than our moon.

The second satellite inwards has an orbit diameter of rather more than two millions of miles. It is a somewhat remarkable circumstance that this satellite, lying between Titan and Japetus, the two largest satellites, should be the smallest or at least the faintest of all Saturn's satellite family. One would be led to suspect that Hyperion is but one member of a zone of small satellites travelling between the paths of Titan and Japetus. It appears to me to be a confirmation of this view that the path of Hyperion does not correspond with the general arrangement of the scheme, but bears somewhat the same sort of relation to it that we should recognise in the orbit of one of the innermost of the asteroids if taken, instead of the zone of asteroids, to represent the orbit intermediate to the paths of Mars and Jupiter.

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* The distinction between our moon and the satellites of the major planets is a marked one, and significant. Our moon is, in point of fact, much more to be regarded as a fifth planet of the inner family of terrestrial planets than as an orb dependent on our earth. If she could be watched from a distant standpoint, her motions would scarcely indicate the fact that she circles around the earth; for, in fact, the result of this so-called circling motion corresponds simply to large perturbational effects. On the contray, the motions of the satellites of Jupiter, Saturn, Uranus, and Neptune, are chiefly influenced by their respective primaries, and this could not but be recognised if the motions of these moons were watched from a distant point. Even if their primaries were concealed during the observation, the motions of the satellites would reveal the fact that these bodies are ruled by a central orb.

As we proceed onwards towards Saturn we are struck with the comparatively close order of the orbits of the inner satellites. The distance separating the orbit of the innermost from that of the fifth Rhea is less than half the distance separating the orbit of Rhea from that of Titan the sixth. The following table of distances has been calculated on the assumption that the sun's equatorial horizontal parallax is 8·916″ :—

	Name.	Distance in mean radii of Saturn.	Distance in miles.	Difference. in miles.
I.	Mimas	3·3607	115,335	32,665
II.	Euceladus	4·3125	148,000	35,250
III.	Tethys	5·3396	183,250	41,490
IV.	Dione	6·8395	224,740	103,100
V.	Rhea	9·5528	327,840	432,150
VI.	Titan	22·1450	759,990	159,180
VII.	Hyperion	26·7834	919,170	1,289,550
VIII.	Japetus	64·3590	2,208,720	

Passing yet farther inwards, after crossing the orbit of Mimas, we come upon the ring-system. The outer edge of the outer ring lies at a distance of 83,460 miles from the centre of Saturn, or 31,875 miles from the orbit of the innermost satellite. It is noteworthy how uniformly the distances from this ring outwards to orbit after orbit of the four inner satellites proceed. This uniformity somewhat resembles what we notice in the case of the four terrestrial planets, since we see that the distance from the sun to Mercury, thence to Venus, and thence to the earth, are very nearly equal (being roughly 35, 32, and 35 millions of miles), while the distance to Mars, though greater, belongs to the same order of distances. The remaining elements, which are convenient for reference in treating of the ring-system, are the following :—

Exterior diameter of outer ring in miles	166,920
Interior ,, ,, ,,	147,670
Exterior ,, inner ring ,,	144,310
Interior ,, ,, ,,	109,100
Interior ,, dark ring ,,	91,780
Breadth of outer bright ring	9,625
Breadth of division between rings	1,680
Breadth of inner bright ring	17,605
Breadth of dark ring	8,660
Breadth of system of bright rings	28,910
Breadth of entire system of rings	37,570
Space between planet and dark ring	9,760

We are thus brought to the planet's globe. Its mass we have already indicated. Its dimensions are as follows:—the equatorial diameter is about 70,150 miles, the polar diameter about $\frac{2}{19}$ths less, or about 63,500 miles. The volume exceeds the earth's 697 times, so that since in mass Saturn only exceeds the earth about 90 times, his mean density is but about $\frac{10}{77}$ths of the earth's. His globe rotates in about 9h. $55\frac{1}{2}$m. on an axis inclined about $26\frac{3}{4}$° to the perpendicular to Saturn's orbit-plane. The rings and all the satellites, except the outermost, travel nearly in the plane of Saturn's equator, but the outermost satellite travels on a path inclined about 15° to that plane.

Even on a general survey, only, of this wonderful system, the impression conveyed to the mind would not be that we have in Saturn an orb belonging to the same order as our earth, were it not for the influence of the preconceptions to which I have already adverted. It seems to me that if we could imagine a visitant from outer space viewing our solar system, he would at once recognise in Jupiter and Saturn the members of an order of orbs probably intermediate in character between the sun and the minor planets. He would reason that while, on the one hand, these planets being very much less than the sun in volume and mass, are obviously of an inferior order, and in this sense resemble the earth and Venus; they are, on the other hand, so far comparable with the sun, even in these respects, that they largely exceed the earth and her fellow planets—that is, in those circumstances in which alone the major planets are comparable with the minor planets, they are as far removed from them on the one hand as from the sun on the other. But when we turn to features in which the major planets resemble the sun, we find that they differ absolutely from the minor planets. Thus, in having the complete control of systems of bodies, these planets resemble the sun, and are utterly unlike the earth. In mean density Jupiter is almost exactly like the sun, and Saturn has even a less mean density than the earth. This last is the most important distinction of all, as we shall presently see; in fact, I think I shall be able to show that of itself it demonstrates the fact that the major planets are utterly unlike the earth and her fellow minor planets. Again, in the condition and phenomena of their atmospheric envelopes, Jupiter and Saturn to a certain degree resemble the sun, as will presently appear; and though the resemblance is not altogether complete, yet this is counterbalanced by the circumstance that the want of resemblance between the major and minor planets in this respect is very marked indeed.

It will be understood that I rely in the main for evidence as to the condition of the atmosphere of Jupiter and Saturn on the results of telescopic researches. Other evidence there is, and in particular the spectroscope has afforded information of an interesting nature. But as yet this information is not definite enough to be reliable as a basis of reasoning; whereas the evidence given by the telescope is sufficient, rightly used, to convey very important information. I would note that we may properly combine the information given by both Jupiter and Saturn, since these planets manifestly resemble each other in those leading features which we can alone deal with in our present enquiries. It is altogether likely that in minor respects Jupiter and Saturn are as unlike as the earth and Mars. But precisely as we can trace a general resemblance between all the members of the minor family of planets, so manifestly Saturn and Jupiter (as also probably Uranus and Neptune) resemble each other in the broader features of their condition. It is very important to recognise this, because, in point of fact, the information conveyed by one planet supplements in an interesting way that given by the other. Jupiter being very much larger and far nearer to the earth can be more satisfactorily studied; and we can only recognise, in the case of Jupiter, the details of those atmospheric belts which girdle both planets. But on the other hand Saturn affords a test as to the nature of these belts, which is wanting in the case of the larger planet. For Jupiter's equator plane is very little inclined to the level in which the planet travels, whereas, as has been mentioned in the elements given above, the equator plane of Saturn is inclined at a very considerable angle, so that what we may for convenience term the seasonal changes of Saturn (noting always, however, that they must be quite unlike the seasons of our earth) are of a marked character, whereas Jupiter has scarcely any seasonal changes at all. Here, then, is a criterion to show what part, if any, the sun plays in producing changes in the condition of the atmospheric envelopes of these planets, since we should expect, if his action is the leading cause of changes, that the changes in the Jovian belts would differ markedly in character from those in the Saturnian belts.

I take for granted the ordinarily described telescopic features of the belts, as presumably known to all the readers of this Journal.

The first feature to be noticed as bearing on the condition of the atmospheres of Jupiter and Saturn, is the remarkable parallelism of the belts. It has been commonly stated that this feature is comparable with the existence of trade-wind zones on our earth. I apprehend that if our earth were viewed from Venus or Mercury, even with high telescopic power, nothing like zones would be recognised. It is, indeed, only over the oceans that the equatorial cloud band exists; and even this is but a mid-day phenomenon. The sun rises in a clear sky in equatorial regions at sea; and it is only towards noon that heavy clouds cover the whole sky. In the afternoon these are dissipated by heavy rain-falls and electric storms, and towards sunset the sky is clear.

There is, indeed, a difficulty in accounting for the zones in the atmospheres of Jupiter and Saturn. Their ordinary regularity implies the existence of a much more effective cause than that which produces our trade-winds. The cause *must* be, it should seem, a difference of rotational velocity, causing clouds to lag as clouds on our trade zones do, only much more markedly, or else causing clouds to be carried in advance of the prevailing rotation-rate. Either cause would serve equally well; the difficulty lies in understanding how either can operate with sufficient activity. The rapid rotation-rates of Jupiter and Saturn would of course make the differences of rate correspondingly great. But so far as the cause which produces our own trade-winds is concerned, this circumstance is much more than counterbalanced by the greatly reduced effects of solar action in Jupiter and Saturn. That, notwithstanding this reduction of effect, sun-raised clouds should be so much more energetically swayed into zones than on our earth must be regarded as altogether improbable. We must find some other interpretation of these zones,—a result which would indeed have been forced upon us, as I think, by the mere circumstance that there are sometimes so many of them. Polar and equatorial air-currents such as exist in our own air would naturally explain, perhaps, an equatorial cloud-zone and sub-tropical trade zones on either side of it; but they can afford no explanation of the existence, even though occasional only, of three or four well-marked zones on either side of the equatorial one.

It appears to me a natural inference from these considerations that the difference of rotational velocity to which the cloud zones of Jupiter are certainly due, does not result from polar and equatorial currents carrying cloud-matter to regions where the movement of rotation is greater or less, but from upward and downward motions within the Jovian and Saturnian atmospheres. Or else, (though the assumption is not incompatible with the hypothesis of upward and downward currents), we may assume that some cause resembling that which occasions the solar spot zones is at work to produce cloud-zones on Saturn and Jupiter. We cannot proceed much farther in this direction until we have more definite information. But it suffices for our purpose to notice that we have in upward and downward currents, combined probably with some resemblance in condition to the sun, a reasonable explanation of the general features of the cloud-zones of the giant planets.

Only, it is necessary to notice that such views require the atmospheric envelopes of these planets to be exceedingly deep, and the upward and downward motions taking place within them exceedingly rapid. The upward motions would, in fact, appear, according to this view, to resemble rather the uprush of vapours during volcanic eruptions than any ordinary forms of vertical currents. The downward motions need not necessarily be so rapid; they might, and probably would, be merely the quiet return of the vapourous masses which had been propelled upwards, by which they resumed the level due to their density.

It appears to me that the results of telescopic scrutiny correspond well with these requirements. It is impossible to observe the belts of Jupiter with adequate telescopic power without being led to the conviction that the atmosphere in which these belts exist is exceedingly deep. It is impossible to convey by any description, and not easy to indicate even by pictorial illustration, the force of the telescopic evidence. It may be remarked, however, that the shapes and changes of shape of the cloud masses show that they have been generated in a deep atmosphere, and therein exposed to modifying influences. There is also one piece of evidence on this point the significance of which is unmistakable. Scarcely ever, *if* ever, is any feature stable, even for a few successive rotations. It is certain that we scarcely ever, if ever, see the real surface of Jupiter or Saturn. I doubt myself whether those features whose successive returns have been observed for the determination of the rotation-rates of these planets are other than atmospheric phenomena. They appear to resemble rather the solar spots, whose motion may indeed suffice for the determination of the general rotation-rate, but cannot possibly be regarded as the motion of some object on the real surface of the solar orb (if he have any). However this be, it is certain that if any part of the real surface of Jupiter has *ever* been seen, the whole of that surface is usually concealed from view. Even the dark zones are not zones of his real surface, though they unquestionably underlie the bright cloud zones. Moreover, signs of violent action such as I have suggested have been by no means wanting; since not only do the features of the Jovian belts change rapidly in shape, but special details are sometimes seen,—as round white spots lasting but for a short period, &c.,—which can be readily explained as due to an uprush of vapour, and, in my opinion, can no otherwise be so satisfactorily accounted for.

But so soon as we admit the probability that the atmospheres of Jupiter and Saturn are very deep, we are led to a line of reasoning which seems demonstrative of the fact that the condition of these planets is utterly unlike that of the earth. A deep atmosphere, subjected to the strong gravitating energy of either planet, would acquire at ordinary temperatures a density altogether incompatible with the existence of any resemblance to our own atmosphere in any part of its extent. In fact, if we only assume that the atmosphere of Jupiter has a depth of 50 miles below the cloud layers, we deduce at the surface of Jupiter a density incompatible with the gaseous state, a density exceeding many times that of our heaviest metals! It is difficult to suppose that that atmosphere, the changes in which are so manifest at our great distance from the giant planets, has a less depth than this would imply; and yet the assumption that it has such a depth leads directly to the conclusion that (if at ordinary temperature) it increases in density till liquefaction is attained, and that if Jupiter has any liquid surface at all, such surface is due to the pressure of the Jovian atmosphere producing liquefaction. There would, however, be more probably continuity of change from the vapourous to the liquid condition. I do not say that this is the actual state of matters; on the contrary, I shall endeavour to show that the real condition of the Saturnian and Jovian atmospheres is altogether different. But this is the conclusion to which we are led if we assume that these atmospheres have such a depth as telescopic observation indicates, and exist at ordinary temperatures.

But if we assume, as we must on the assumption of ordinary temperature, that, at a comparatively moderate depth below the apparent limits of the globes of Saturn and Jupiter, the liquid condition is attained by mere vastness of atmospheric pressure, or else exists simply because the atmosphere is not so deep as we have been supposing, we have, in the small mean densities of Jupiter and Saturn, a problem of no ordinary difficulty. We cannot possibly believe (in the presence of the results experimentally shown to follow from applying great pressure to solid materials) that there can be cavernous openings in the interior of these vast globes. Under enormous pressure, solids, even such solids as iron, gold, and platinum, are perfectly plastic. They *run* like fluids. And no pressure ever yet applied experimentally is for a moment comparable with the pressure which must exist at very moderate depths below the surface of Jupiter and Saturn. It is as incredible that cavernous openings or great hollow interiors exist in the solid globes of Saturn and Jupiter as it would be that cavernous openings should exist in the depths of ocean, and with no other walls but the water itself. If the globes of Saturn and Jupiter are in the main solid or liquid, they are solid or liquid throughout, without gaps or interstices. Moreover, they are subject to a pressure constantly increasing from surface to centre, and enormously greater than that at the earth's centre, at but a relatively short distance from the surface. In Jupiter especially, the pressure must increase with great rapidity, on account of the greatness of his attractive power; for gravity at his surface exceeds gravity at the earth's surface fully 2½ times. And again, in Jupiter's enormous globe, a depth such as that which separates the centre of the earth from her surface is relatively insignificant, being less than a tenth part of the distance separating his centre from his surface, on our present assumptions. That a solid or liquid globe, subjected to such enormous pressures throughout by far the greater part of its volume, should have a mean density less than a fourth of the earth's, as in Jupiter's case, or less than a seventh of the earth's, as in Saturn's, must assuredly be regarded as a most surprising circumstance. Of what materials should such a globe be composed? Are there any materials whatever, of such small density, which can be supposed to exist in such relatively enormous quantities as to constitute nearly the whole of the mass of Jupiter or Saturn? It would be absurd to regard as a reasonable hypothesis, *now*, either the theory of Whewell, that Jupiter consists mainly of water, or the alternative suggestion of Brewster, that the substance of the giant planets may be of the nature of pumice-stone. Such theories as these are the really startling theories of science; they are really fanciful, because based on no known physical facts. They were unscientific even when they were propounded; but they are doubly unscientific now that spectroscopic analysis has rendered it probable that the elements constituting the great orbs of the universe are in the main the same that we are familiar with on earth, and proportioned similarly as to relative quantity. If our sun were to solidify, there would be in his globe our familiar iron, gold, copper, calcium, sodium, and other metallic elements, combined with those other elements which we recognise as the chief constituents of the earth's globe. We cannot refuse to admit the probability that what is true of the small earth and the gigantic sun, as well as of his giant brothers, the stars, can scarcely fail to be the case with the intermediate order of bodies to which Saturn and Jupiter belong. But we cannot admit this inference, probable though it is, if we adhere to our assumption that the conditions of temperature in Saturn and Jupiter resemble those in our earth. We should certainly find these planets, in that case, as dense as our earth, or rather (considering the much larger pressures to which the greater part of their mass would be subjected) we should find their mean density very much greater. Since, on the contrary, they are of very small mean density compared with the earth, we are driven from the assumption that their physical condition resembles that of the earth.

It is natural, under these circumstances, to inquire whether, since the earth gives us no satisfactory explanation of the condition of Jupiter and Saturn, we may not find in the sun some suggestions towards an explanation. It would have been natural, indeed, to have turned first to the sun, because of the much greater similarity of condition already mentioned as existing between the giant planets and the sun. But I preferred to consider first the less obvious and probable line of argument, partly to dispose of it, and partly because, unlikely and even unreasonable as it is, it is the one which has hitherto been nearly always followed. Very few have thought of explaining Jovian and Saturnian phenomena by a reference to solar phenomena; very many, including believers as well as disbelievers in the habitability of Saturn and Jupiter, have endeavoured to force the observed phenomena into accordance with the familiar phenomena of our own earth.

We notice at once that the same low density being recognised in the case of the sun as we have been discussing in the case of the giant planets, any explanation which presents itself in the sun's case is available, at least to be tested by whatever evidence may exist.

Now we can have no hesitation in ascribing the sun's small mean density to the great temperature of his whole globe. This temperature operates against those effects of pressure which, in his case, would operate far more markedly than in the case of the giant planets. Many elements, which in his interior would be solidified or liquefied by the great pressures to which they are subjected, remain gaseous, and others which do not remain gaseous yet exist at a density far lower than that which they would have if at ordinary temperatures. We may say of the sun as a whole that its globe is expanded, and so reduced in density by excess of heat. We are not able to explain precisely how his various elementary components are disposed throughout his globe in consequence of the temperatures and pressures at which they severally exist. In fact, everything in the sun is so unlike what we are familiar with, that we cannot apply directly any of the known laws of physics to the interpretation of solar phenomena. But we remain, nevertheless, satisfied that the main reason why gravity has not its will throughout the solar orb, compressing the substance of that orb until a high mean density results, is that the great heat of the sun opposes the process of contraction which would operate at once if gravity were left unresisted.

The inference is that the orbs of Jupiter and Saturn are in like manner intensely heated, though, it need hardly be said, by no means to the same degree. Nor, in fact, can it be necessary, to maintain the mean densities of Saturn and Jupiter at their present low amount, that anything like the same degree of heat should exist in their case as in the sun's. For the pressures due to gravity are far less even near the surface of these planets, and the distance from surface to centre, on which the amount of the increase of pressure beneath the surface depends, is very much less. But that the temperature of Saturn and Jupiter greatly exceeds that which exists in the case of our own earth, seems to be the explanation to which we are driven by the facts of the case, and, from what we know of the sun, may be regarded as a sufficient explanation.

It remains to be seen, however, whether this explanation is supported or negatived by other facts. If it be the correct explanation, moreover, not only should it be in accordance with the facts hitherto considered, but it should indicate the existence of other relations than those which have led us to it. In other words, on carefully considering this theory, consequences should appear to follow from it which correspond with the actual results of observation.

Let us begin, however, by considering the arguments which at a first view seem to oppose the theory that the globes of Saturn and Jupiter are intensely heated.

It is manifest that the heat required by the theory is such that the substance of the real globes of Saturn and Jupiter must be self-luminous, and perhaps to a high degree. Now it is certain that the discs of these planets, as we see them, do not owe their light chiefly to inherent luminosity. This is shown, not only by what I shall mention presently as to the quantity of light received from these planets, but also by a circumstance which deserves more careful attention than it has yet, I think, received. Not only does the disc grow darker near the edge,* as it would if the light were reflected light, but both Jupiter and Saturn, when near their quadratures, show a marked defalcation of light on the side where lies the terminator of the really gibbous disc. Again, we see that the satellites of Jupiter disappear in the shadow of their primary, which would not be the case if the planet shone with a very strong inherent light. An even clearer argument at a first view is found in the circumstance that the shadows of the satellites look black, as though the *whole* of Jupiter's light were reflected sunlight.

None of these circumstances, however, suffices to prove that the disc of Jupiter does not possess some degree, and even it may well be a considerable degree, of inherent luminosity. It is manifest that the vanishing of the satellites in Jupiter's shadow would only imply that his disc does not glow with an intense lustre of its own, a fact which is otherwise obvious. The darkening at the edge of the disc does not show that the light is *in the main* reflected, though it proves that a considerable share of the planet's light is reflected. The apparent blackness of the satellites' shadows would be a strong argument if we were not aware how large a part contrast may play in producing such a phenomenon. The spots on the sun look black at their nucleus by contrast with the light of the photosphere, but so does the glowing lime of the oxyhydrogen light look black against the sun's disc. And a like test is available to show that the blackness of the shadows of Jupiter's satellites may be apparent only. For the satellites themselves ordinarily look dark in transit, and the fourth satellite looks absolutely black. Now this fourth satellite is certainly not black; in fact, it shines on the background of the sky with a sufficiently bright light. We have, therefore, no evidence that the inherent luminosity of Jupiter may not be equivalent to the light which the fourth satellite reflects.

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* To ordinary vision, the reverse appears the case; but the reason of this is that the dark sky on which, as on a background, the disc is seen, causes the edge of the disc to look dark by an effect of contrast. Mr. Browning tested the matter two or three years ago with a graduated darkening-glass, and found that, as had been theoretically inferred from the aspect of the satellites on different parts of the disc, the edge is darker than the middle.

Now it is to be noticed that the light of matter glowing with intensity of heat is not necessarily intense. The red of burning coals, for example, or of hot metal is not so bright as many imagine. It is not so bright as a red object in ordinary sunlight. The very best imitation of a fire of glowing coals I ever remember to have seen was produced by a piece of rather dark red cloth, accidentally placed in a grate in an ill-lighted room (day-light). Bright red cloth in sunlight does not produce nearly so good an illusion; and it is perhaps hardly necessary to remark that the ordinary imitations of a coal-fire in theatrical representations fail in consequence of their being very much too bright.

In my opinion, there is nothing to prevent us from assuming that the darker belts of Jupiter owe the ruddiness of their colour to the glow of a red-hot interior, and this extends to the case of Saturn, whose dark belts bear a tolerably close resemblance to those of Jupiter in their general tint. I would not have it thought, however, that I limit the inherent heat of Jupiter to ordinary red heat. I simply infer that the portion of the inherent light which makes its way through the cloud-laden envelope of the dark belts corresponds to that of red-hot matter. I should regard the brighter belts, notwithstanding their greater brightness, as those whence less inherent luminosity proceeds, the greater part of their light being, I conceive, reflected from clouds of purest white, concealing the glowing surface beneath. It is to be carefully noted, however, that we have no reason for believing that any part of the cloud-belts of either planet is absolutely continuous. Openings two or three hundred miles across would be quite invisible from the earth with the best telescopes and the most piercing eye-sight. In fact, the apparent area of such openings in Jupiter's atmosphere would be less than the fiftieth part of the least of Jupiter's satellites.

All that we might expect, I think, as an effect of the high temperature which our theory requires in the giant planets, is that these globes should shine with a light notably brighter than globes of equal size, similarly placed, and constituted of some such substance as the whiter kinds of sandstone. Now this is certainly the case. The lowest estimates of the brightness of Jupiter and Saturn assign to these orbs regarded as absolutely opaque a reflective power more than twice as great as that of white sandstone. Jupiter's brightness, as a whole, is not far inferior to that which he would have if his whole surface were of the whiteness of driven snow. When we remember how far his globe is from being white, its actual whiteness as a whole resulting from the combination of several colours, we see that, according to this the lowest estimate, he must possess some inherent lustre. But other estimates have placed his brightness far higher. And it is a notable circumstance that Dr. De la Rue, in photographing Jupiter and our moon under the same circumstances (atmospheric and otherwise), found that the actinic power of the moon is to Jupiter's only as 6 to 5, or 6 to 4, whereas if the two bodies both shone only by reflecting solar light, and possessed equal reflective powers, the moon should have nearly 27 times the actinic power of Jupiter, since Jupiter is 5⅕ times further from the sun than the moon is. "On December 7, 1857, Jupiter was photographed in 5 seconds, and Saturn in 60, and on another occasion the moon and Saturn were photographed in 15 seconds, just after an occultation of the planet." Jupiter should exceed Saturn about four times if both shone by reflecting solar light, and Saturn would appear, from the observation of December 7, 1857, to be of a brightness inferior to that due to its greater distance, while the other observation would imply the reverse. It seems manifest that photographic results would require a careful comparison, not only *inter se* but with some standard, to lead to satisfactory conclusions.

We may place greater reliance on direct photometric estimates; and, as it seems to me, the following results by Zöllner, when carefully studied, indicate that the condition of the outer planets differs essentially from that of the earth, Mars, Moon, and probably Venus and Mercury. He found the light of the following planets, regarded as light-reflecting bodies, indicated the tabulated reflecting powers :—

Moon	0·1736
Mars	0·2672
Jupiter	0·6238
Saturn	0·4981
Uranus	0·6400
Neptune	0·4648*

The following determinations of the reflecting powers of terrestrial substances indicate the significance of Zöllner's results :—

Snow just fallen	0·783
White paper	0·700
White sandstone	0·237
Clay marl	0·156
Quartz porphyry	0·108
Moist soil	0·079
Dark grey syenite	0·078

I have discussed at some length and in various aspects, in my "Other Worlds" and Essays on Astronomy, the evidence in favour of occasional actual change in the shape of Saturn. It seems to me that it may now be desirable to quote the *ipsissima verba* of Sir W. Herschel as to the so-called square-shouldered aspect of Saturn. It must be mentioned in the first place that no possible question can now exist as to the ordinary shape of Saturn's disc being that of an ellipse. Herschel himself was not quite sure whether the square-shouldered aspect might not have been presented even when he made those earlier measurements which seemed to indicate a truly elliptical figure. But as we now know from the measurements of Main, Bessel, and others that the ordinary figure of Saturn is elliptical, we see what interpretation can alone be placed on the observations now to be quoted, if accepted.

It is customary to assign April, 1805, as the first occasion on which Herschel perceived any departure from the elliptical figure. But he himself quotes the following observation, made 17 years earlier :—

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* I may note, by the way, that, considering the enormous difficulty of determining the dimensions of the planet Neptune, it may be questioned whether the above result might not suggest that the real dimensions of Neptune are less than usually stated, and therefore the *albedo* greater than above indicated. Of course it would be extremely rash to assume this on the strength merely of the fact that Neptune has a less albedo than any of his fellows among the giant planets. Still the point is worth noting.

"August 2, 1788, 21h. 58m. 20-feet reflector; power 300.—Admitting the equatorial diameter of Saturn to lie in the direction of the ring, the planet is evidently flattened at the poles. I have often before, and again this evening, supposed the shape of Saturn not to be spheroidical (like that of Mars and Jupiter), but much flattened at the poles, and also a very little flattened at the equator; but this wants more exact observations."

The results observed in 1805 have been often quoted by myself and others. It appears, therefore, desirable to proceed to the results obtained in 1806:—

"April 16, 1806.—I examined the figure of the body of Saturn with the 7 and 10-feet telescopes, but they acted very indifferently; and, were I to judge by present appearances, I should suppose the planet to have undergone a considerable change. Should this be the case, it will then be necessary to trace out the cause of such alterations."

"April 19. 10-feet; power 300.—The polar regions are much flattened. The figure of the planet differs a little from what it appeared last year. This may be owing to the increased opening of the ring, which in four places obstructs now the view of the curvature in a higher latitude than it did last year. The equatorial regions, on the contrary, are more exposed to view than they have been for some time past."

Then follow several observations indicating a close resemblance in 1806 to the figure which the planet had presented in 1805, when the flattening was first recognised. At length we have:—

"May 9. Power 527.—The air being very clear, I see the figure of Saturn nearly the same as last year; the flattening at the poles appears at present somewhat less; the equatorial and other regions are still the same."

These observations, combined with those made in 1805, and with subsequent observations by Schröter, Kitchener, Sir John Herschel, Coolidge, the Bonds, Airy, and others, seem to leave little doubt as to the occasional apparent expansion of the planet in its temperate zones, and also as to other changes of figure sometimes limited to one hemisphere of the planet's globe.

I find it difficult to understand how these observations, made, be it observed, by experienced astronomers, can be explained as due to illusion. They accord perfectly well with the theory which I have advocated in the present essay and elsewhere. I would not indeed suggest that owing to any processes of expansion or contraction changes take place in the real globe of Saturn, or even that his atmosphere becomes at times heaped up in particular regions; but there is nothing to prevent the occasional existence (perhaps for long periods of time) of cloud-layers at higher levels than usual in the temperate zones, or else the occasional dissipation of the higher cloud-layers in the equatorial zone.

I have described, in my "Other Worlds," an observation of the ingress of one of Jupiter's satellites on the disc, and its subsequent reappearance, *as though it had retraced its course;* and I have shown that the only conceivable explanation of this remarkable phenomenon (witnessed by three excellent observers at different stations) appears to reside in the rapid dissipation of a high cloud-layer. I have never seen any other explanation even suggested; and, for my own part, I cannot agree with those who would simply abandon all attempt at explanation.

The general conclusion to which all the evidence, as it seems to me, would appear to point, is that both Jupiter and Saturn are in a semi-sun-like condition. It is not altogether correct to say that they occupy a position midway between that of the earth and that of the sun, for, in point of fact, such a mode of expression does not admit of any definite interpretation. It is manifest, moreover, that in some respects Jupiter and Saturn are utterly unlike the sun, while in other respects they are utterly unlike the earth and her fellow terrestrial planets. They must be regarded as *sui generis;* and it must be the work of long and careful observation with the best telescopes to ascertain the nature of these orbs. This is a subject of independent research, and although some analogies suggested by our knowledge of the earth, and other analogies suggested by our knowledge of solar phenomena, may be useful as guides, yet, on the whole, the safest course will be to pursue the inquiry in an independent manner. And here I would note that there is excellent promise of new information to be derived from the systematic observation of Saturn and Jupiter in both hemispheres of the earth.* Hitherto most of the observations have been made in the northern hemisphere, though even there systematic observations have been wanting. Now Jupiter for six years and Saturn for fifteen years lie to the south of the equator, and are therefore ill-placed for observation from northern stations. It may well be that changes occur having as their period the year of either planet, in which case observations made during one half only of the year of either planet cannot reveal the law or nature of such periodic changes. And in any case it must needs be unsatisfactory that trustworthy observations should be interrupted for periods so long as those I have named.

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* It would not, indeed, be necessary that northern and southern observatories should be engaged in the work if an observatory could be established, for researches into the physics of astronomy, in some elevated region of our possessions in British India. For, near the equator, the ecliptic at all times passes high above the horizon. It was proposed some time since, by an almost unanimous vote of the council of the Astronomical Society, that such an observatory should be erected for the purpose indicated. Whether this proposal will be carried out remains to be seen. It unfortunately was made only as an amendment on a proposition of an eminently unsatisfactory

A similar consideration applies to the Saturnian rings. It seems to me to have been demonstrated that these rings consist of multitudes of discrete bodies, though whether these be solid, fluid, or vapourous remains uncertain. Observations of both sides of the ring-system are much required, however, to elucidate the whole subject of the constitution of these strange objects. Now hitherto only the northern side of the system has been satisfactorily examined; and this side is only presented towards us under conditions favourable for study during two or three successive oppositions out of the whole series of oppositions occurring during a Saturnian year. It is worthy of remark that all the chief discoveries respecting the rings have been made at these times. It cannot but be regarded as most desirable that the opportunities afforded when the ring is most open, but the southern side turned earthwards, should not be lost. If, as is supposed, the ring-system is undergoing processes of change, systematic observations at these favourable times are essential to the inquiry into their condition.

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nature, brought forward by Colonel Strange; and it is to be feared that it may not be received with such favour in Government circles as it would perhaps have obtained if not found in questionable company. Nevertheless it is in itself an excellent proposition, and, supported as it was by all the leading members of the council except one, it is difficult to imagine that Government would refuse a favourable hearing to it. Should it ever be carried out, we can scarcely doubt that the physics of astronomy would be importantly advanced, and results obtained which at present are unattainable owing to the limited range of our northern observatories.

II. ON THE RELATION BETWEEN REFRACTED AND DIFFRACTED SPECTRA.

By MUNGO PONTON, F.R.S.E.

TO all workers with the spectroscope, an accurate method of determining the relation between the indications of that instrument and the normal positions of the spectral lines in the diffracted spectrum, which correspond to their wave lengths, has long been an object of desire.

The method hitherto followed, in ascertaining the wave-length corresponding to any line found in the field of the spectroscope, is that of interpolation, by means of the formula suggested by Mr. W. Gibbs, in "Silliman's Journal," for July, 1870. Reduced to its most simple shape, this formula may be stated thus:—Let a, b, c represent the given positions of any three lines in the index of the spectroscope, and let x, y, z represent their corresponding wave-lengths. Further make $b-a=p$, $c-b=q$, and $c-a=r$. Then, according to the formula of Mr. Gibbs, we have—

$$\frac{p}{z^2}+\frac{q}{x^2}-\frac{r}{y^2}=0$$

from which any one member may be found if the rest are given.

This formula, however, is applicable only when the three lines are very near each other—a proximity not always attainable. Moreover, it will be found that, even when the three lines are near, the result is much less approximately correct in some parts of the spectrum than in others.

The truth is, that the formula is absolutely correct only when thrown into the following more general shape—

$$\frac{p}{z^\varepsilon}+\frac{q}{x^\varepsilon}-\frac{r}{y^\varepsilon}=0$$

where the exponent ε is variable, having diverse values in different parts of the spectrum.

The question thus arises, whether it may be possible to ascertain the law, according to which this exponent varies, with sufficient accuracy to render the formula available, not only for determining unknown wave-lengths, but also for correcting those already approximately ascertained by observation. Could implicit reliance be placed on an adequate number of observations, this task would not be difficult.

At present, however, the observations are neither so numerous nor so correct as to admit of the determination of the law with perfect accuracy. It is nevertheless possible, by taking advantage of the observations as they exist, to determine the manner in which the exponent varies with such a degree of correctness as may be found available for practical purposes.

The following investigation has been based on a careful comparison of Ångström's Atlas of the Diffracted Spectrum, and his Relative Tables of Wave-lengths, with the Maps and Indices of the Refracted Spectrum, by Kirchhoff, Hofmann, Ångström, and Thalén.

To find the value of the exponent ε for any three lines, of which both the positions in the index of the spectroscope and the corresponding wave-lengths are approximately ascertained by observation, is a little troublesome; because it can be done only by the method of gradual approximation, or trial and error. It therefore becomes of importance to have the value of ε determined and tabulated for every 10° of Kirchhoff's scale; because the value of ε being known, the calculation of the wave-length from the general formula becomes easy. Such a table must proceed on the assumption that the *rate* of variation of the value of ϵ remains nearly uniform for 10° of Kirchhoff's scale; and although this assumption cannot be said to be absolutely correct for every 10°, yet it is so nearly accurate as to be practically available.

For the purpose of framing such a table, it is convenient to assume the extreme lines A, and the more refrangible H of the spectrum as constants; so that, in the formula, the quantities x, z, and r may be always the same. Then if b, the index position in the spectroscope of any line, be given, its corresponding wave-length y may be found by means of the tabular value of ε in the region where b is situated. It is further convenient to assume the value of z to be 10, and to determine the wave-lengths, in the first place, in relation to this basis, converting them afterwards into the millimetric scale of Ångström. By this method we have always $\varepsilon = \log.\ x_\varepsilon$, which is a facility in calculation.

The assumed position of A in Kirchhoff's scale is 404·5, and of the more refrangible H 3882·5, the difference being $3478 = r$ in the formula. Then the value of p is always $= b - 404{\cdot}5$, and that of $q = 3882{\cdot}5 - b$. The log. of the wave-length x in relation to z is 1·2863197, and the log. of that log. is 0·1093490, which is one of the constants in the calculation. The log. of r is 3·5413296, and is another constant. The log. by which the relative wave-length *y* found by the calculation, is converted into the millimetric scale of Ångström is 2·5947234.

The following are the values of ε for the six principal lines of the spectrum, intervening between A and the more refrangible H, based on their positions in Kirchhoff's index, and the formulated values of their wave-lengths relatively to that of H = 10.

Index position.		Log. of wave-length.	Log. of log.	Value of ε.
B.	593·1	1·2420499	0·0941391	3·1516
C.	694·1	1·2223188	0·0871845	3·1963
D.	1002·8	1·1757690	0·0703210	3·2322
E.	1523·7	1·1269776	0·0519153	3·0057
F.	2080·0	1·0919797	0·0382145	2·6244
G.	2854·4	1·0394751	0·0168141	3·3636

From the last column it will be perceived how variable is the value of ε, and how erroneous must be any formula which is based on the assumption of its being constant—more especially if the constant be fixed so low as 2.

The annexed table of the value of ε has been calculated for every 10° of Kirchhoff's scale, for the entire space between B and H. In the space between A and B, the observations are so few and uncertain that it has been found impossible to extend the table into this region with any degree of satisfaction.

A careful examination of this table will show that, were the values of ε graphically represented, they would form not a continuous curve, but a waved line—although in certain parts the line becomes straight. From 590 to 1000 of Kirchhoff's scale, the value of ε gradually rises from 3·1516 to 3·2342; but there is a slight interruption to the continuity of the ascent at 830 of the scale, where it has attained to 3·2130. Here it remains constant for 5 terms, after which it falls slightly, becoming at 880 only 3·2040. It then ascends until, at 930, it reaches 3·2310, from which point there is a second fall, until at 960 it becomes 3·2210, whence it ascends to the maximum of 3·2342, which it reaches at 1000 of the scale.

From this maximum there is a continuous and more or less regular descent, until at 1980 of the scale the value of ε is reduced to its minimum of 2·6150. From this point there is a continuous ascent to a second maximum at 3526, where ε reaches the value of 3·4520. In this interval there are several points, at which ε remains unaltered for several successive terms of the series. From 3526 of the scale there is again a descent, which attains its lowest limit at 3770, where ε is 2·9100. This value remains constant to 3820 of the scale, from which point there is a rapid ascent to the end of the series; but in this part of the table the values of ε are somewhat uncertain.

In some parts of the series, the differences in ascending or descending from term to term are pretty regular; while in other parts these differences vary more abruptly. Their general characteristic feature is an alternate rise and fall in their value, so that, were they represented graphically, they would exhibit a very wavy line, at intervals becoming straight.

For this table, absolute accuracy is not claimed; but it is as nearly correct as it can be made in the present imperfect state of the observations. Future more accurate observations may render it necessary to introduce into it slight modifications here and there; but it is far from probable that these will affect its main features; while it may even now be trusted in the calculation of the wave-lengths, which will be correct to the fourth place of figures in Ångström's millimetric scale.

To illustrate the method of applying the scale to this purpose, let us take an example. Suppose we wish to ascertain the wave-length corresponding to the hydrogen line, which stands at 2796·7 of Kirchhoff's scale. One advantage of taking the extreme lines of the spectrum as constants is, that it presents the general formula always in one shape, namely—

$$y^{\epsilon} = \frac{r}{\frac{p}{z^{\epsilon}} + \frac{q}{x^{\epsilon}}}$$

where y is the wave-length to be found, and the other quantities are all given.

In the case of the above hydrogen line, we have—

$p = 2796{\cdot}7 - 404{\cdot}5 = 2392{\cdot}2$ log. $3{\cdot}3787975$
$q = 3882{\cdot}5 - 2796{\cdot}7 = 1085{\cdot}8$ „ $3{\cdot}0357498$
r constant $3478{\cdot}0$ „ $3{\cdot}5413296$

The value of ε corresponding to index 2800 is 3·2890
and to index 2790 3·2790

Difference 0·0100

so that 3·2790 + 67 = 3·2857 is the tabular value of ε, corresponding to 2796·7 of index.

TABLE.

K	ε	K	ε	K	ε	K	ε	K	ε	K	ε	K	ε	K	ε
590	3·1516	990	3·2330	1360	3·0611	1730	2·8158	2100	2·6380	2470	2·9475	2840	3·3500	3490	3·4319
600		1000	342	70	568	40	068	10	460	80	570	50	614	3500	
10	526	10	270	80	527	50	2·7977	20	550	90	670	60 to	664	10	
20	556	20	200	90	487	60	886	30	640	2500	770	2900		20	324
30	606	30	131	1400	449	70	795	40	720	10	850	10 to	680	30	330
40	676	40	064	10	414	80	704	50	800	20	930	40		40	380
50	736	50	000	20	381	90	612	60	880	30	3·0030	2950	710	50	450
60	796	60	3·1935	30	350	1800	519	70	970	40	130	60	750	60	520
70	856	70	873	40	322	10	427	80	2·7070	50	240	70	780	70	440
80	910	80	814	50	298	20	337	90	150	60	340	80	790	80	350
90	951	90	758	60	280	30	249	2200	220	70	450	90		90	260
700	980	1100	704	70	270	40	164	10	310	80	550	3000	820	3600	170
10	3·2006	10	652	80		50	081	20	400	90	660	10	860	10	080
20	030	20	601	90	260	60	000	30	490	2600	780	20	880	20	3·3980
30	052	30	551	1500	230	70	2·6917	40	540	10	890	30	890	30	780
40	072	40	502	10	170	80	834	50	590	20	990	40		40	480
50	090	50	454	20	090	90	749	60	670	30	3·1070	50	950	50	130
60	105	60	407	30	000	1900	660	70	760	40	150	60 to	3·4000	60	010
70	117	70	361	40	2·9910	10	570	80	860	50	260	90		70	3·2960
80	126	80	316	50	820	20	480	90	970	60	370	3100	010	80	930
0 to	130	90	272	60	729	30	390	2300	2·8070	70	480	10	020	90	730
830		1200	229	70	636	40	310	10	140	80	600	20	045	3700	300
40	120	10	187	80	541	50	240	20	200	90	720	30 to	080	10	3·1620
50		20	146	90	445	60	190	30	270	2700	830	70		20	3·0940
60		30	106	1600	349	70	160	40	340	10	940	80	110	30	310
70	100	40	067	10	253	80	150	50	410	20	3·2060	90	160	40	2·9780
80	040	50	029	20	157	90		60	490	30	180	3200	190	50	430
90		60	3·0992	30	062	2000	160	70	600	40	290	10 to	250	60	210
900	100	70	955	40	2·8968	10	170	80	720	50	390	70		70 to	100
10	150	80	919	50	875	20	180	90	830	60	490	80	260	3820	
20	215	90	883	60	783	30	190	2400	920	70	600	90 to	270	30	200
30	310	1300	848	70	692	40	200	10	990	80	700	3370		40	500
40	290	10	813	80	602	50	210	20	2·9050	90	790	80	280	50	3·0100
50	230	20	777	90	513	60	221	30	120	2800	890	90	290	60	3·1100
60	210	30	739	1700	425	70	232	40	200	10	3·3010	3400 to	300	70	2600
70	290	40	698	10	337	80	244	50	290	20	160	3470		80	4700
80	310	50	655	20	248	90	300	60	380	30	340	80	304		

To find $\frac{p}{z^{\epsilon}}$—

log. p 3·3787975

ϵ 3·2857

$\frac{p}{z^{\epsilon}}$ = 1·239075 0·0930975

to find $\frac{q}{x^{\epsilon}}$ log. of log. x 0·1093490

log. of ϵ 0·5166279

log. x^{ϵ} 4·2264610 0·6259769

$\frac{p}{z^{\epsilon}}$ 1·239075 log. q 3·0357948

log. x_{ϵ} above 4·2264610

$\frac{q}{x^{\epsilon}}$ = 0·064466 - - - $\bar{2}$·8093338

log. r 3·5413296

$\frac{p}{z^{\epsilon}} + \frac{q}{x^{\epsilon}}$ = 1·303541 log. 0·1151247

log. y^{ϵ} = 3·4262049 log. 0·5348133

log. ϵ 0·5166279

log. y = 1·0427624 0·0181854

add constant 2·5947234

y = 4339·96 3·6374858

According to Ångström, the value of this wave-length in the millimetric scale is.	4340·10
According to the above it is	4339·96
Difference	0000·14

which is almost unappreciable.

In judging of this result, it must be borne in mind that the chances of errors of observation, in ascertaining the wave-length and the position of the line in the scale of the spectroscope, are about equal; so that, of the above difference, small as it is, only one-half should be set down to the wave-length. If the latter be made 4340·03, thus halving the difference, then with the exponent $\epsilon = 3·2857$, the position of the line in the scale of the spectroscope would have to be made 2796·56, which differs only 0000·14 from that assigned to it by observation—a difference which is in like manner scarcely appreciable. As determined by the table, then, this hydrogen line ought to have for its index position 2796·56, and for its wave-length 4340·03.

The mode of finding the value of ε, when both the position of the line in the scale of the spectroscope and the wave-length are given, and held to be absolutely exact, differs but little from the foregoing. A certain value of ε must be assumed, and when log. y^{ε} and its log. are found as in the preceding calculation, the log. of the log. of y, as given by observation, is to be subtracted from it. If the value of ε has been accurately assumed, then the difference should be exactly equal to log. ε. If the difference be greater than this last, then the assumed value of ε must be increased; if the difference be less, then the value of ϵ must be diminished. A little experience will show the extent of the requisite increase or diminution, which varies in different regions of the spectrum.

Thus, in the foregoing case, if from the log. of log. y^{ε}	0·5348133
We subtract log. of log. of the relative wave-length corresponding to 4340·1 of the millimetric scale	0·0181911
We have	0·5166222
Which is less than log. ε by	0·0000057
Log. ε	0·5166279

Thus showing that the value of ϵ should be slightly diminished. The requisite diminution is 0·0015, making the exact value of ϵ, on the assumption that the observations are absolutely correct, 3·2842.

It will hence be perceived how very sensitive is the exponent, and how considerable a variation it may undergo even with a scarcely appreciable difference of wave-length, or in the position of the line in the spectroscopic scale.

In the table (p. 31), the columns marked K contain the degrees in Kirchhoff's scale; while those marked ϵ contain the corresponding values of the variable exponent. The table is submitted in the hope that, notwithstanding any slight imperfections it may be found to contain, it may prove useful to those who are engaged in spectroscopic investigations.

III. OBSERVATIONS ON THE OPTICAL PHENOMENA OF THE ATMOSPHERE.

By S. Barber, F.M.S..

DURING the first quarter of the year 1872, many remarkable optical phenomena were observed in the neighbourhood of Liverpool; and as I have on previous occasions published remarks bearing on the prognostic value of these appearances,* it seemed advisable to collect and compare such notes as may bear upon the prediction of weather, and help to elucidate the questions that have been raised as to the origin, and varieties of halos and parhelia. The subject, certainly, is one of the highest interest, both in relation to the various crystalline forms of water and the constituent particles of the various forms of cloud. A beam of light passing through the upper atmosphere may reveal to us, by the laws of refraction, the degree of congelation of vapour floating ten or twenty miles away. The investigation of the cirrus and cirro-cumulus being admittedly of the highest importance in relation to the movements of the great atmospheric currents, it seems surprising that the optical laws and phenomena bearing on their constitution should not have received more attention at the hands of meteorologists.

Before passing on to the proper subject of this paper—viz., halos and parhelia—I may remark that the rainbow, which appears to have been so thoroughly investigated, exhibits at times more variety, both in form and colouring, than most writers seem to be aware of. These varieties will be found—like those of the halo—to have a relation to the general atmospheric conditions of the time. For example, I have frequently noticed that the tendency to an irregular outline, and the appearance of double bows, &c., are mostly connected with stormy and squally weather. A dark day in autumn, with low-flying nimbo-cumulus, and driving rain, will sometimes exhibit, in its transient gleams of sunshine, most curious and unexpected forms. On such occasions, though the outlines are more remarkable, the colouring is not so brilliant and decided as in calmer weather.

In the last "Quarterly Journal of the Meteorological Society," Mr. Scott gives notes of a double rainbow, with reversed colours; and Mr. Lecky adds an account of a triple one produced by reflection from the surface of water—seen of course in calm weather. I may add to these an observation of a bow, which was almost devoid of colouring, and divided into separate rings, four or five in number, concentric, and decreasing in width towards the centre,* so that the innermost was almost invisible. These rings were near together; and the originating cloud seemed of low altitude.

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* Quarterly Journal of Science, No. 26.

On another occasion I have observed a bow in which the outline was decidedly broken and unsymmetrical. This was also in the cumulus cloud, and in stormy weather.

I now pass on to the forms of halo and mock sun; two of the latter were seen during the winter and spring of 1872. The first was imperfect in definition, as seen from Aigburth, about 4½ miles south of Liverpool. This, though not a brilliant form, is worthy of record from the fact of its having been seen at stations widely distant from each other. It was described in the "Times," as seen at Meath, in Ireland. On comparing notes, I found that this description agreed in several points with the observation made by myself. The mock sun as it appeared near Liverpool was of an oval form, and situated vertically over the real sun. It lay at the point of contact of two arcs of halos tangent to each other. There was no cloud in the sky, only a slight haze, such as occurs after heavy dew and hoar-frost. This occurred about 9.10 on the morning of Jan. 22. There had been a slight frost during the night.

The phenomenon having been observed at several places some hundreds of miles apart, the originating crystals must have been of unusual altitude and extent. In passing, I may remark that the height of mock suns is probably very inconstant. Thus, in the cold weather of spring (1871), I was fortunate enough to take an observation of the sudden formation of a well defined ball of prismatic light, which owed its existence to the passage of a rapidly moving *cumulus* cloud. This could scarcely have been higher than 5 or 6 miles at the most. The mock sun was very similar, both in form and position, to that described by me in the "People's Magazine" (February, 1872)—and it appeared and disappeared with the cumulus cloud. The weather changed to heavy rain on the day following the mock sun of Jan. 22, 1872.

The next instance, of which I took more careful notice, was visible from the Mersey, at Liverpool, from about 11.50 till 12.15 at noon, on Wednesday, April 10th. (See sketch on next page.) It seems to me to be a very unusual form. The wind was nearly N. at the time, and the weather cold. As I have ventured to maintain that halos and kindred optical phenomena, generally indicate a transitional state of the temperature or of the weather, and not, as is popularly supposed, an approaching storm, I draw attention to this case as an important, though by no means isolated, confirmation of the theory. It followed many days of *wet*, and preceded about a week's *dry* weather. On referring to the sketch it will be seen that an arc of a halo attended the mock suns (which were not very brilliant), and that the chief peculiarity of the case lay in this—that the mock suns were clearly external to the halo, and at a distance of 1° 30′ or 2°. The halo showed chiefly a red colour, and the parhelia were entirely of a light prismatic blue, very distinct from that of the surrounding sky.

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* The outer ring was about half the width of an ordinary rainbow, and the inner ones became narrower by a fixed proportion.

Exactly a week after the last mentioned phenomena were seen, a solar halo again occurred about 1 p.m., Wednesday, April 17, and after this the weather grew warmer and the sky still clearer. We shall probably not be justified in expecting a *decided* change after every appearance of these phenomena, for I have often observed that, in showery and changeable weather, two or three solar halos of varying degrees of definition will occur on successive days; but for several years I have found that, when there is anything very singular in the form or combinations of the circles, changes of a decided character often follow.

Upon examining the recorded notices of these appearances, we find that there are usually more accounts of lunar than of solar halos, yet I have no doubt, from my own observations, that more *solar* halos really occur, and that most of them are overlooked on account of the dazzling effect of the sun's rays, rendering their observations somewhat troublesome to those who have not a strong eyesight. I can safely say that in the neighbourhood of Liverpool they greatly preponderate in number. Scarcely any change, indeed, from wet to fair, as well as from fair to wet, occurs without their appearance. Two more years' observations enable me to endorse the statement concerning their prognostic value, which I published in spring, 1871.* I desire now, however, to draw attention to two peculiarities connected with the lunar halo which appear to be of very evil portent. M. W. de Fonvielle, in commenting on an appearance of very rare form of lunar halo, of which I had sent an account to "Nature" (Jan. 26, 1871), makes the following remark:*—"Il est presque inutile d'ajouter que l'apparition de ce halo a été suivie, comme d'ordinaire, d'une chate de neijes abondantes." Of course this remark refers to the winter season. I am not aware, however, that any case is on record of a halo of 90° appearing in the summer time. Other similar cases of equal prognostic value have since been observed. One of the most remarkable occurred during the autumn of 1871.

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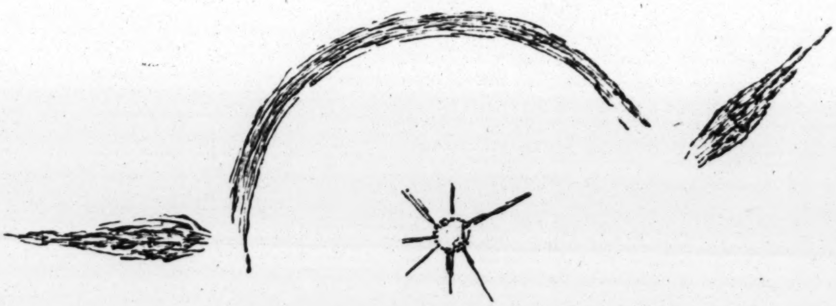
* Nature, March 23, 1871.

This was a long horizontal band of light that proceeded in a straight line from the moon to a distance equal to the radius of an ordinary halo. At the point where this line would have intersected the halo there were two short bands of light passing through it vertically, so as to form a kind of cross laid horizontally in the sky. Another phenomenon, which, I imagine, has a kindred origin, was observed by me, near Liverpool, on the Sunday before Easter, 1872. On this occasion the cross was formed at the moon itself, and the bands were only two or three degrees in length, at right angles to each other. After these appearances I took particular note of the weather. In a day or two, very heavy rain set in, and continued for a long period. Students of Meteorology will long remember the summer of 1872, and it is remarkable that it should have begun with so many and such unusual optical phenomena. This fact, however, is particularly noteworthy—that after the great electrical disturbance set in, and till the termination of the storms, not a single instance of a well defined halo or any

FIG. 1.



Parhelia. April, 1872.

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* Comptes Rendus, Fevrier 27, 1871.

cognate optical appearance was seen; and equally remarkable was the absence during the same time of the cirrus and cirro-cumulus forms of cloud. It would appear that the amount of electricity diffused through the atmosphere had some influence on the congelation of vapour, or that the forms of cloud in question are in some way dependent on an electrical condition, or a relation of the upper to the lower stratum of vapour. During this season the electric cumulus, of various forms and shapes, was the prevailing cloud form.

Professor Poey, in his recently published remarks on cloud classification, points out that, during the most pronounced rain seasons, the two kinds of "pallium,"—viz., the high ice-pallium and the low mist-pallium—are separated by a neutral region of the atmosphere, and that the pallio-cirrus is then in a state of negative electrical excitement, in common with the air at the surface of the ground; while the pallio-cumulus and the rain that issues from it are in positive electrical excitement; and that electrical discharges continually take place between the two concomitantly with the pouring down of *unelectrified* rain from the lower stratum upon the earth.

These results are singular and interesting, though it is to be regretted, perhaps, that Prof. Poey has not given some details of the means by which they were obtained. It has seemed strange to me that the invention of atmospheric electrometers and electroscopes, in combination with the Captive balloon, has not been put to some practical use in the prediction of weather. By placing several of these instruments at various altitudes, and synchronously noting their indications, further important results may perhaps be obtained.

On referring to Buchan's "Handy Book of Meteorology," I find a case of a halo intersected by twelve rectilinear bands of light proceeding from the sun. No remark is made on the origin of the bands; but experiment seems to show that we must attribute these to reflection, and look for the cause in the configuration of the terminating facets of the suspended crystals. This theory is corroborated by the fact that these bands of light are achromatic, which would not be the case if they were caused by refraction.

A halo broken in its outline, and caused by well-defined bands of linear cirrus, is, I believe, a more certain sign of rain than a continuous circle. Another form that I have noticed before bad weather is a fleecy and irregular circle, wider in some parts than in others; it is of a very evanescent character, and has but little colouring. This form, which is very uncommon, appears sometimes when the sky is almost clear.

A few notes on recent appearances of parhelia, in relation to the attendant halos, may not be inappropriate. The sketch given of *blue* parhelia accompanying a *red* halo, and clearly external to it, illustrates, I think, a very uncommon case. It will also be seen that, while most mock suns have an elliptical or oval outline, and the major axis in a line with the *circumference* of the halo, the major axis of this lay in a line with the *radius* of the halo, and the balls of light were not symmetrically placed.

Cases of parhelia without any halo are very rare. An instance may be found recorded in "Nature," of an oval mock sun entirely within the attendant circle, and situated vertically above the real sun. Most of the cases of parhelia noted by me have been in a horizontal line, not a vertical one; and those situated above the sun have been very deficient both in colouring and definition of form.

One remarkable feature of these appearances is this, that when the parhelia are seen in very perfect form, the halo that accompanies them appears only to show one colour, and that generally red I believe; whereas the ordinary solar halo has blue for the most part predominant.

I now pass on to consider the various media, or conditions of the atmosphere that give rise to these optical phenomena. The form of cloud denominated by Howard cirro-stratus is, as he himself remarked, the most frequent originator both of lunar and solar halos; but ordinary cirrus also often produces them in an imperfect form. The normal or "curling" type of cirrus seldom shows prismatic tints, though we know it to be strictly an ice-cloud; nor does the form of cumulus which we may call "snow cumulus." This seems strange when we consider that the halo is simply a result of ice refraction; but it may be explained upon the hypothesis that, in these two forms of clouds, the particles are so massed together, and the prisms and crystals so overlaid one upon another, that the refracted rays are again combined and a white line produced, as may be seen on a close inspection of a fresh snow-flake.

Taking, therefore, the various forms of cloud, we find that halos are usually restricted to the following:—

1. Cirro-stratus, or as it is sometimes called linear cirrus.

2. Ordinary cirrus. On this form I may remark, that it is when somewhat thin and transparent that the halo appears.

It would be well, perhaps, to make distinction between the kind of cirrus here alluded to and the whiter and denser variety, which approaches in character to cirro-cumulus. This variety is evidently a different composition, from the fact of chromatic phenomena being absent. The surface is whiter and less marked, and the outline more distinct than in the halo-producing variety.

3. "Club" cirrus. I thus denominate the long narrow line of cloud which proceeds from a kind of tuft partaking of the nature of cirrus and cirro-cumulus.

These are the commonest forms of *cloud* that produce halo. We now take plain skies. I have observed the phenomenon under the following conditions :—

1. A light greyish blue. This is very unusual.

2. A deep opaque blue. On this occasion the halo was entirely red, as when the parhelia appeared.

3. High thin mist (probably floating crystals). This is not uncommon in frosty weather. There is a general law with regard to these phenomena which may be stated here, viz., that, "the clearer the sky the more perfect is the colouring." This I have verified by many cases. It may also be added that the more defined the clouds of the upper stratum, the less likely are halos to be seen.

IV. RECENT CHANGES IN BRITISH ARTILLERY MATÉRIEL.

By S. P. OLIVER, Capt. R.A.

I. EARLY this year experiments have been carried out both at Shoeburyness and on board H.M.S. *Excellent* with some modified 24-pounder Hale's rockets to test their range, accuracy, and incendiary power in comparison with the ordinary service rockets, Mark III.

The modified rockets have the internal form of the cone in the composition altered and a modified tail piece. These alterations were expected to have the effect of greatly increasing the velocity, duration, and rapidity of rotation, ensuring greater range, greater accuracy, and less tendency to puff. The modified rockets were found to be (with one exception) all steady in flight, whilst all the service rockets puffed more or less. Those fired from the *Excellent's* cutter with Fisher's rocket apparatus gave good results, with the exception of two, which burst prematurely in the air and blew their heads off. Their incendiary power was tested as follows:—Their heads were filled with Carcass (Valenciennes) composition. Four rockets of each nature were firmly fixed in a hole bored in a balk of timber, their heads protruding and butting against another balk covered with inflammable material. One of the rockets, after the composition had burnt out, burst explosively, and blew the head to pieces before the Carcass composition could be ignited. Mud was heaped upon the heads of the others while the composition was burning and the flame forced its way through; water was also poured upon the heads without extinguishing the flame.

Capt. Boys, R.N., considers that the incendiary powers of the new modified rockets over those of the service are considerably increased—that their range is rather better. The deflection of both rockets is equally variable, the maximum of service rockets being 100 yards, and that of the experimental rocket 80 yards on either side of the target. The Lords Commissioners of the Admiralty therefore consider the modified rockets to be preferable for naval service; the defect of premature explosion can easily be remedied.

II. The Electro-Ballistic experiments under Capt. Noble have been continued, and tables have been arranged showing the comparative probable rectangles of various ordnance, and the greatest velocities obtainable by different guns. The highest mean muzzle velocities obtained from the 9-pounder and 16-pounder rifled muzzle-loading guns, using service common shells and service R. L. G. powder, were found to be respectively 1562 and 1466 feet per second.

III. The report on the traversing arrangements are satisfactory. After practice two men can easily traverse a 9-inch gun from right to left (arc about 110°) in 45 seconds. Some of the 9-inch platforms are being tried with traversing gear on the spur- and mitre-wheel principle.

With regard to the recoil of guns, the Elswick compressors have been found to work most satisfactorily, and are to be retained where already fitted to 7 and 9-inch gun platforms.

IV. Important experiments and results have been carried out and obtained by the Committee on gunpowder and other explosives, especially as regards battery charges.

In determining the battery charge for any gun, the proper course to be followed, in the opinion of the Committee, is as follows:—To increase the charge gradually until distinct wave pressures are exhibited; the highest charge which can be employed without these local pressures appearing should then be accepted as the battering charge. Experience has clearly demonstrated that, by limiting the charge in accordance with this rule, the maximum useful effect is attained without any risk of undue pressures in the gun ; and that if the charge thus fixed is exceeded, a portion of the powder is wasted, and the gun rendered liable to undue local pressures. The charge of 85 lbs. of pebble powder exceeds that which the above-mentioned rule would give for this gun, but to no dangerous extent, although the maximum useful effect of the charge is not obtained.

In determining the battering charge, a comparison between the power of the gun with a calibre of 11 and 12 inches came incidentally under the notice of the Committee; the results of the experiments clearly demonstrated that a gun of 145 inches length of bore is more powerful as a battering weapon with a 12-inch calibre than with one of 11-inch, and this is still more evident when it is considered that with a 12-inch calibre the gun would probably consume 95 lbs. of powder with as good useful effect per lb. of powder, and with no greater pressure per square inch than it does 85 lbs. of powder with an 11-inch calibre.

The detail has been published of various experiments with 35-ton rifled muzzle-loading gun, No. 1. Shot, 700 lbs.

The pressures were determined by crushers fitted by means of a copper cup at the bottom of the bore (A), by a screw gauge inserted instead of the vent plug (B), and by a gauge screwed into the base of the projectile (C). After round 8 the gun was fired by an electric tube placed in the cartridge at 12 inches from the bottom, the wires passing through a groove in the shot to the muzzle.

The following are a few of the points elicited by these experiments, which appear of the greatest general interest :—

1. If the powder be burned uniformly in the gun, without any indication of wave action, the pressure increases with the increase of charge, at first rapidly, but after 20 tons on the square inch has been exceeded, then very slowly. In the whole course of the Committee's experiments a uniform pressure by crusher gauge of 30 tons in the powder chamber has never been attained; this fact appears strongly to corroborate the experiments carried out by Captain A. Noble, at Elswick, on the pressure produced by ignited powder in closed vessels, which indicated that the maximum pressure produced by ignited powder in a perfectly closed space is somewhat less than 40 tons to the square inch.

2. When a charge of any description of powder is increased beyond a certain limit, wave or local pressures are set up which strain the gun unduly, without affording an equivalent of useful effect on the projectile.

3. Provided the battering charge is not exceeded, the pressure in the gun increases steadily with the increment in weight of the projectile up to a certain point; beyond this point no material increase of pressure can be obtained by increasing the weight of the projectile.

4. Proof of pebble powder of Waltham Abbey and trade manufacture in accordance with specification.

This proof has been carried on with the 8-inch smooth-bored gun prepared for the purpose. The Committee consider the results to be on the whole very satisfactory, and express their unanimous opinion that the velocity and pressure test of powder for heavy guns should never be suspended, as they are satisfied it is the only proof that will ensure the supply of powder good and uniform in quality.

Considerable difficulties have been experienced, both at Waltham Abbey and by the merchants, in keeping the powder up to the specification; this might be reasonably expected in the production of a new article. The Committee consider themselves justified in saying that the progress made is encouraging, most of the difficulties having been overcome. Valuable knowledge is daily acquired, and light thrown on many points by the system of proof adopted on their recommendation.

V. Another explosive picric powder consists of only two ingredients, saltpetre and picrate of ammonia, the ingredients being incorporated in the same way as those of gunpowder.

The perfect stability of the two ingredients, *per se*, even if exposed to degrees of heat very far beyond the extremes of tropical temperatures, has long been fully established.

Picric powder is certainly not *more* susceptible of explosion by friction or percussion than gunpowder.

Its exploding point has been compared in several ways with that of gunpowder. In some instances the picric powder exploded at a somewhat lower temperature, or a little more readily, than gunpowder; in others the reverse was the case. The two substances may, therefore, be considered to have about the same exploding point.

Samples of picric powder, prepared early in 1870, which have been exposed to light and preserved at ordinary atmospheric temperatures, are perfectly unchanged.

Samples have been exposed for several days successively to 212° Fahrenheit without sustaining any change.

Samples have also been heated for several hours daily, during several days, to a temperature ranging from 300° to 320° Fahrenheit. The picrate of ammonia was slowly volatilised from the powder by this treatment, just as the sulphur would be from gunpowder. In other respects the powder was unchanged.

A sample of picric powder, which had not been submitted to pressure, was exposed in an atmosphere saturated with moisture for 18 days, when it had absorbed 14 per cent of water; it was then exposed to the open air at the ordinary temperature (September, 1870), and was found to have parted with the whole of the water absorbed.

At the end of 20 days another sample was exposed to the damp atmosphere until it had absorbed 5 per cent of water in 6 days; upon subsequent exposure day and night to the air it returned to its original weight in 8 days. Its exposure to the open air day and night was afterwards continued for 40 days, in the months of September and October, and its weight ascertained early each morning. The maximum increase in weight during the experiment was 6 per cent. On the 40th day, as on several other days during the experiment, it had returned to its original weight, and its properties were unchanged.

Not the slightest indication has been obtained, in very searching experiments, that picric powder is in any respect more prone to change than gunpowder.

Further trial of picric powder will be combined with the trial of gun-cotton pulp as a bursting charge for shells.

In the opinion of the Director of Artillery, picric powder has the disadvantages of gunpowder as regards danger, and, on the other hand, is not equal to gun-cotton as to power or safety.

The quantities of picric powder already tried have been small, and it has not as yet developed qualities to lead to the conclusion that it will either supersede gunpowder or gun-cotton.

Considering, therefore, that as the qualities of gunpowder are known, and those of gun-cotton are being developed, he thinks it would be premature to introduce picric powder into the service at present, but that it had better remain in an experimental stage until its actual properties and comparative safety are more fully known.

But the Director of Naval Ordnance considers it desirable that further experiments should be carried out with picric powder, as it may prove a valuable explosive for shells and the Harvey and outrigger torpedoes, where the amount of charge is limited.

VI. The following experiments have been made by the Special Committee on Explosives upon an explosive substance termed "Pyrolithe," which is being manufactured in the town of Middlesborough, to determine whether it is of such a character as to bring it within the meaning of section 6 of Act 23 and 24 Vict., c. 139:—

1. 1 lb. tin gunpowder canister (obtained from Messrs. Curtis and Harvey) filled with pyrolithe taken from No. 1 cask.

The canister was laid upon the ground, and the material ignited by means of a piece of Bickford fuze placed in a hole in the side.

The contents burnt with considerable violence, but without exploding; the solder of the case melted almost immediately, and the pyrolithe burnt fiercely from the apertures thus caused until consumed.

2. 1 lb. tin gunpowder canister filled with pyrolithe taken from No. 2 cask.

The nose of the canister was placed on the ground to prevent it from being immediately blown off, and a small piece of iron was placed on the canister to weight it, and to represent the condition of one package in a box, or under others.

The material was ignited (as in the previous case), and exploded with a dull but decided report, and with sufficient force to project the canister (which was ripped open) to a distance of fourteen yards.

3. 1 lb. tin gunpowder canister filled with pyrolithe from the cask *not marked*.

The results were the same as those obtained in No. 1 experiment.

4. 1 lb. tin gunpowder canister filled with pyrolithe from the cask *not marked*.

This canister was placed in a fire of wood and coals laid in a brazier. In about four minutes the pyrolithe ignited and burnt fiercely, but without exploding. The solder of canister was melted.

5. Small wooden keg (with a capacity to contain 5 lbs. of gunpowder) filled with pyrolithe from No. 1 cask and headed up.

The keg was ignited by means of a piece of Bickford fuze introduced through a hole in the side. On the pyrolithe taking fire the head of the keg was blown out with a slight report, and the material then burnt rapidly away.

6. Small wooden keg filled with pyrolithe from No. 2 cask.

In order to prevent the head of the keg being too readily blown out, and to represent the condition of barrels stacked one on the other, a weight of about 50 lbs. of iron was placed on the head.

The pyrolithe on being ignited blew the head and the weight off with a decided report, and having thus found a vent burnt rapidly away.

7. Quarter casks, 1, 2, and 3 (containing about 20 lbs. of pyrolithe in each) headed up, placed in original packing case, and then covered over with shavings and wood.

In a few minutes after the shavings and wood were ignited, one cask caught and burnt violently; in about 15 seconds more the second cask caught and the flame became more violent; and in about 30 seconds the third cask caught, and caused an almost explosive burst of flame; the whole then burnt with considerable fierceness until consumed.

On full consideration of the results the Committee are of opinion that under conditions which might arise in connection with the manufacture, transport, or storage of pyrolithe, its ignition would be followed by a more or less violent explosion, and consequently the character of this substance comes within the meaning of the Act quoted.

Samples of pyrolithe from each of the three casks were forwarded to the Chemist to War Department for analysis, and the following are the results obtained:—

Description of Sample.	Soluble matter consisting of nitrates, carbonate of soda, and sulphate of soda.	Composition.		
		Sulphur.	Saw-dust.	Percentage of moisture.
No. 1, P. $\frac{5}{\text{S.M.}}$	70·94	16·00	13·06	9·01
No. 2, P. $\frac{\text{8 C.}}{\text{P.}}$	73·08	16·12	10·80	8·88
No 3, not marked	69·89	16·36	13·75	8·88
Old sample which had been in Major Majendie's possession for some time.	70·94	16·14	12·92	3·07

VII. Complaints, it appears, are frequently made with regard to the irregularities in burning of Boxer wood time-fuzes, especially in mountain batteries, under varying atmospheric pressure above the sea-level.

The whole subject of the influence of local altitude on the burning of time-fuzes has been carefully investigated both practically and theoretically,—

Practically by Quartermaster Mitchell, in India, in 1849, at different altitudes, viz.:—

St. Thomas's Mount . . .	Sea level.
Bangalore	3000 feet.
Kotagheny	6500 „
Cotacamund	7300 „

Theoretically by Professor Frankland, in 1860, and subsequently, practically, by the French Academy of Science, at altitudes, viz.:—

Oucby	1246 feet.
St. Pierre	5380 „
Chenallettis	9481 „

The agreement in these different and distinct observations was most remarkable, and from them was deduced the following practical rule:—

That the time of burning of a fuze increases in the ratio of 0·0011 of its value for each diminution of 0·0394 inch of barometrical pressure; or, in other words, of about 0·03 of its value for each variation of 1 inch pressure. Atmospheric pressure diminishes almost uniformly at the rate of 1 inch for every 1000 feet of altitude; hence the time of burning of a time-fuze increases 0·03 of its value for each increase of 1000 feet of altitude, or 0·003 of its value for each variation of 100 feet of altitude.

Thus if a 9-seconds fuze burns exactly 9 seconds at the sea level, it will burn 11·16 seconds at an altitude of 8000 feet above the sea. The times of burning of the fuzes at the sea level will, in future, be placed in the cylinders, together with a notification that "The time of burning increases nearly 3 per cent for every 1000 feet of altitude."

IX. It is most important that an efficient gas-check should be provided in order to prevent the erosion in the bores of muzzle-loading rifled guns which is caused by the scoring rush of gas over the top of the shot, more especially when firing with pebble-powders. Various tin cups and cow-hide wads, &c., have been tried, at present without success, and the service wad is discontinued as useless.

X. Colonel Inglis's muzzle derrick will be adopted for

9-inch guns and upwards when mounted in open batteries and *en barbette*.

XI. The 400-lb. shell, common 10-inch, when fired from the 18-ton gun, has been found sufficiently strong to penetrate the sides of wooden unarmoured ships without breaking up ; but with regard to the 7-inch projectile from the 90-cwt. gun, the Committee come to the following conclusions:—

1. That the present experiment affords no trustworthy evidence of the relative destructive effect which would probably be produced by common shell after passing through the side of wooden and iron unarmoured vessels. It is worthy of note that after passing through the side of the wooden target, a shell, if it does not break up or explode at once, is liable to turn sideways. Under such circumstances the projectile would probably lodge, and might act as a mine in the opposite side. Exact information on this point cannot well be obtained without firing at a decked structure, or an actual vessel.

2. So far as destructive effect is concerned, the Committee are unable to form any trustworthy estimate of the comparative value of any of the projectiles fired.

The special 7-inch projectile has the advantage of an increased bursting charge, but this appears to entail a loss of strength in the shell, and, looking to the inconvenience of adopting a new pattern, the Committee, on the whole, prefer the service shell, weighing filled 115 lbs.

3. The distance at which common shell break up or explode after passing through the side of a vessel, either of wood or of iron, depends in a great measure on the nature of the resistance met with. If the shell hits on a knee, a rib, or a diagonal iron brace, it almost invariably breaks up or explodes in passing through the side ; if, on the other hand, it passes fair between two ribs in a place where the resistance is confined to the wood planking or iron plating, it may not break up or explode until it has passed from 6 to 10 feet from the side.

Complete information on these points cannot, however, be obtained without practice at an actual ship.

4. That the projectiles fired from both guns were liable to break up (without bursting) in passing through either of the targets. The shells appeared to break up whether they were filled with sand or with gunpowder ; in the latter case the bursting charge in several instances merely fired. The full effect of the explosion did not appear to be realised unless the shell struck on a part of the target where, owing

to increased resistance, the onward velocity was suddenly checked.

XII. The Moncrieff system of mounting guns has been tried successfully with guns up to 7 tons weight, can be applied with advantage to 9-inch guns of 12 tons, and possibly extended to guns of 18 tons and upwards.

The 9-inch carriage subjected to trial was of the improved type, known as Pattern II. It differs from the original construction adopted in the case of the 20 7-inch muzzle-loading Moncrieff carriages made for service, and from the first experimental carriage for 9-inch guns of 12 tons, in the following particulars:—

1. The carriage proper is dispensed with, and the gun rests directly on the elevators. By this arrangement, the strain which, in the carriages of original construction, was received upon the rear axletree at the moment of firing, is now conveyed through the vertical elevating bars to a grooved stool bed upon which these bars slide. By this means the blow which previously was met directly is now gradually absorbed.

2. The gun comes down into a constant position for loading, whatever elevation or depression it may have in the firing position.

3. It is simpler and more compact, although heavier, but it affords greater facility for loading and elevating.

Notwithstanding the increased cost of the Moncrieff as compared with the service carriage and platform, his system (an ordinary barbette unprotected battery always excepted) is considerably cheaper than any other, constructed of either earth or stone protected with iron shields. As compared with the cost of a casemated battery, with shield, as estimated for a 9-inch gun of 12 tons, the balance in favour of the Moncrieff system would amount to about £1800 per gun, while, as compared with that of an open battery, with shield, with and without splinter-proof cover, the saving would be respectively £450 and £667 per gun.

As regards economy and efficiency, therefore, the Committee consider the Moncrieff system compares very favourably with that of the service, especially when it is considered that, from its extensive lateral range, one gun mounted on a Moncrieff carriage may do equal work with two or more guns mounted behind shields.

The system will be found particularly well adapted for,—

(1.) Mounting guns in salients, &c., of land defences, and—

(2.) Mounting guns for subsidiary defence of existing heavy sea batteries; they allude more particularly to such works as Picklecombe, Bovisand, &c., the guns of which being essentially armour piercers, should have associated with them guns of lighter calibre for shell fire.

(3.) The defence of the great commercial harbours.

The expense of mounting a few 12-ton or possibly heavier guns on Moncrieff carriages would be considerably less than placing them behind shields or in casemates; while the increased protection afforded to the men over that of guns *en barbette* would be a matter of great importance.

With regard to the employment of the Moncrieff system for mounting guns of large calibre on sea defences, the Committee consider that it might be resorted to with advantage, but the extent of its application necessarily depends upon local and other considerations, of which they can have no cognizance.

Should it, however, be contemplated to project new works for the defence of important positions, or to supplement existing works by others of the present type, the Committee are strongly of opinion that the designs should be re-considered with a view to the employment of the Moncrieff carriage.

XIII. The manufacture of 8-inch rifled muzzle-loading howitzers for the service is being proceeded with.

1. As a howitzer, this piece will be usually employed in destroying earth works, in breeching unseen defences, or in shelling buildings, ships, &c.

In these operations the elevation will seldom exceed 20°, and, as a rule, high charges will be used, the shells being designed to burst on impact, or by means of a percussion fuze.

2. As a mortar, it will be used in bombarding magazines or other bomb-proof buildings; in dropping shells upon the decks of vessels; in dislodging troops from cover, or in destroying *matériel* behind cover, &c.

In these operations the elevation may vary from 20° to 40°, or even higher angles, and the charges from the highest to the lowest. As a rule, however, low charges will seldom be used, except in dislodging troops from under cover, and under these circumstances a time fuze will generally be found most effective.

Elongated projectiles falling at angles of 50° to 70°, or under conditions of vertical fire, will enter the ground for several feet before a percussion fuze will have time to act; thus the effect of the explosion will be comparatively slight in a lateral direction.

The effect of shells burst in the air, over the heads of troops, or just in clearing the parapet, would be much more searching than the effect of shells which had entered the ground before exploding.

Again it is an open question which nature of fuze would be best when firing at bomb-proof structures.

It is possible that under these circumstances the wood time fuze would act percussively, but by cutting it long the shell might be given time to enter to its greatest depth for exploding; it would thus act with the greatest advantage as a "mine."

XIV. From experiments with guns fired from casemates, and behind shields at Picklecombe, Bovisand, and elsewhere it has been found—

1. That a slight difference in protrusion of muzzle of gun has an immense effect with regard to concussion and smoke, which are much lessened.

2. That the mantlets materially lessen the amount of smoke and concussion in casemate, but not sufficiently so to allow of many contiguous guns being worked at close interval when firing rapidly.

The side pieces or wings are somewhat cumbrous to move; do not allow sufficient play for bringing the mantle up to the gun when trained at an angle; and are in the way of men loading when the gun is trained at any considerable angle.

3. The solution of chloride of calcium to render the rope uninflammable answers admirably.

XV. The experiments with the 35-ton guns have been also satisfactory. Some difficulties have been experienced in loading when the recoil is less than five feet, and it is necesssary for one of the gun detachment to hold up the end of the rammer outside the work, the leverage of the stave being too great for Nos. 2 and 3 to support it within.

The shooting of the common shell of 618 lbs., with full charge of 85 lbs. of pebble powder, is better than that of the Palliser shell of 700 lbs., and battering charge of 110 lbs., which is principally due to the shearing of the front studs of the latter and consequent increase of gyration, which causes inaccuracy and a want of uniformity in range and deflection.

Meantime improvements, both in armour-plated ships and armour-piercing guns, continue to be made, and whilst at

Portsmouth the double turreted *Inflexible* is being built with 20-inch plates for her citadel at Woolwich, a sixty ton experimental gun, with calibre of 15 inches, to throw a projectile of 1100 lbs. weight, is in progress. This new gun we learn is fitted with a breech-loading apparatus, but no details have yet been published.

V. THE GEOLOGICAL SURVEY OF THE UNITED KINGDOM.

AS the applications of science to industry are every day becoming more important, it may be interesting to review the origin and progress of our National Geological Survey. This institution was established for the purpose of arranging, in a form easily accessible to the public, a complete body of information respecting the geological structure of the British Islands, and the disposition and extent of their mineral wealth.

It was about forty years ago when Sir Henry (then Mr.) De la Beche proposed to the Government to publish copies of the ordnance maps geologically coloured. This proposal being acceded to, the Survey was commenced single-handed by him, in the year 1834. Having for some time previously worked at the geology of the west of England, he was the better prepared to issue geological maps of Cornwall, to which his attention was first given. Subsequently, a small branch of the Trigonometrical Survey (then under the superintendence of Colonel Colby, R.E., F.R.S.), was formed under the directorship of De la Beche.

About the same time, a geological branch of the Ordnance Survey was formed in Ireland, and placed under the charge of Captain Portlock.

In 1835, De la Beche suggested to the Chancellor of the Exchequer that a collection should be formed, and placed under the charge of the Office of Works, containing specimens of the various mineral substances used for roads, in constructing public works or buildings, employed for useful purposes, or from which useful metals were extracted. In 1837, the sanction of the Treasury was given to this design, and a building in Craig's Court was devoted to the work of the Office and the reception of the specimens. This was replaced by the more suitable building now occupied at Jermyn Street, the Museum of Practical Geology, which was opened to the public in 1851.

In Ireland, the specimens were first formed into a museum at the Ordnance Survey Office in Belfast, and afterwards transferred to Dublin, where they are now placed in the Museum of Irish Industry.

In 1845, the Geological Survey was transferred from the Ordnance Survey to the charge of the Chief Commissioner of Her Majesty's Woods, Works, and Land Revenues; and an Act of Parliament was passed, giving the necessary powers to all duly appointed officers of the Survey to examine every portion of the country without fear of being prosecuted as trespassers on private property.

Professor A. C. Ramsay was appointed the first Local Director for Great Britain, and Captain (now Sir Henry) James for Ireland, acting under De la Beche as Director-General. Meanwhile the Office of Woods and Forests was modified, and, on the formation of the Department of Science and Art in the year 1854, the Geological Survey of the United Kingdom was consigned to it, at first under the Board of Trade, and afterwards under the Committee of Privy Council for Education.

Since this period, great changes have taken place in the direction and organisation of the Survey, while the staff has been largely increased. The Director-General, Sir Henry De la Beche, died in 1855, and was succeeded by Sir Roderick Murchison, who died in 1871. Professor Ramsay then received the appointment, and Mr. Bristow was made Director for England and Wales. Captain James (now Director of the Ordnance Survey) was succeeded in Ireland by Dr. Oldham (now Superintendent of the Geological Survey of India), Dr. Oldham by Mr. Jukes, who died in 1869, and he by Mr. Hull. Scotland, too, has been severed from England, and Mr. Geikie appointed Director.

As at present constituted, therefore, the Geological Survey of the United Kingdom is under the Director-Generalship of Professor A. C. Ramsay, LL.D., F.R.S. Mr. H. W. Bristow, F.R.S., &c., is Director for England and Wales; Mr. Edward Hull, M.A., F.R.S., is Director for Ireland; and Professor Archibald Geikie, LL.D., F.R.S., for Scotland. The field-staff embraces two district-surveyors, eight geologists, twenty-four assistant-geologists, and one fossil collector, in England; one district-surveyor, three geologists, nine assistant-geologists, and two fossil-collectors, in Ireland; and one district-surveyor, two geologists, six assistant-geologists, and two fossil collectors in Scotland. In addition to these officers, Mr. R. Etheridge, F.R.S. (Palæontologist), and Professor T. H. Huxley, LL.D., F.R.S. (Naturalist), with two assistants, have the naming and arranging in the museum at Jermyn Street of the fossils collected on the Geological Survey of England and Wales.

The results of the Survey operations will be learnt from the published maps, memoirs, and sections. The following statistics show the present state of the progress of the Geological Survey.

The whole of Wales has been completed on the one-inch scale, while in England twenty-five counties have been finished. The area which remains to be surveyed comprises portions of Northumberland, Cumberland, Westmoreland, Durham, Yorkshire, Lancashire, the Isle of Man, Lincolnshire, Nottinghamshire, Leicestershire, Cambridgeshire, Huntingdonshire, Norfolk, Suffolk, and Essex. In England and Wales, which comprise 110 sheets, 80 complete sheets have been published on the one-inch scale; while numerous maps on the scale of six inches to one mile have been published to illustrate the coal-fields of Yorkshire, Northumberland, Durham, and Lancashire. A number of sheets adjacent to the Yorkshire coal-field, and not intended for publication, are deposited for reference at the Geological Survey Office, where they can be seen, and (under certain conditions) copies may be obtained. Portions of the western counties, Gloucestershire and Somersetshire, have been re-surveyed in greater detail.

In Ireland, which comprises 205 sheets on the one-inch scale, 135 sheets have been published, and what remain to be finished comprise portions of Galway, Mayo, Roscommon, Sligo, Leitrim, Fermanagh, Cavan, Monaghan, Tyrone, Donegal, Londonderry, Antrim, Down, Armagh, and Louth. All these maps were surveyed on the scale of 6 inches to a mile, and reduced for publication. Altogether seventeen counties have been completed.

In Scotland, which comprises 120 sheets on the one-inch scale, 18 maps have been published, illustrating the geology of portions of Wigtonshire, Ayrshire, Kirkcudbright, Dumfriesshire, Lanarkshire, Renfrewshire, Peeblesshire, Dumbartonshire, Stirlingshire, Linlithgowshire, Edinburghshire, Haddingtonshire, Berwickshire, Fife, and Kinross. Maps on the 6-inch scale have been published to illustrate the coal-fields of these counties.

Numerous horizontal sections drawn to the scale of 6 inches to the mile, and vertical sections, on a scale generally of 40 feet to an inch, have been published to illustrate the geological structure.

Memoirs and Explanations, containing accounts of the stratigraphical relations of the rocks, their characteristic fossils, and notes on the mines and minerals accompany most of the maps. Special memoirs on large areas, and detailed descriptions of fossils have also been published.

From three to four thousand square miles are annually surveyed in the United Kingdom. There is, however, much old ground to be gone over in mapping the superficial deposits, which not only have an important economic value in many instances, but are also intimately connected with questions of health, of drainage, and water-supply.

The Museum at Jermyn Street well illustrates the applications of geology, by exhibiting a series of rocks and minerals, and their adaptation to purposes of use and ornament. An extensive palæontological collection likewise illustrates the geological maps; the study of fossils proving an important guide in the identification of strata.

It is by studying the Maps, Sections, and Memoirs together, that the great practical value of the Survey is understood. The Maps themselves will show the superficial extent of the different strata, whether gravel, sand, clay, limestone, slate, sandstone, marl, or alternations of these rocks, such as clay and limestone, sand and gravel. The colours representing these geological formations are an indication of position and age. To learn their thicknesses, mineral characters, &c., the Memoirs must be consulted; while to understand their underground extension, the Sections will prove necessary.

Hence the applications to Agriculture, Engineering, and Architecture, and still more to Mining, will be at once apparent.

It is needless to remark upon the fruitless trials for coal which have been made even in recent years. The late Professor Jukes has stated that the money wasted in such searches, of which he had been personally cognisant, could not have been less than £150,000. The Geological Survey has checked much of this fruitless expenditure.

Some of the more important results of the Survey are shown in the Report of the Royal Coal Commission. The area of the exposed coal-measures of England is estimated at about 2840 square miles. The investigations of Professor Ramsay have led him to conclude that 3141 square miles of coal measures are present beneath the Permian and Triassic strata—301 square miles more than the area of our exposed coal-fields!

VI. GALL'S DISCOVERY OF THE PHYSIOLOGY OF THE BRAIN, AND ITS RECEPTION.

By T. Symes Prideaux.

"Strictly speaking you only play the part of puppets in a show; when certain cerebral organs are put in action, you are led according to their seat to take certain positions, as though you were drawn by a wire, so that we can discover the seat of the acting organs by the motions. I know that you are blind enough to laugh at this; but if you will take the trouble to observe, you will be convinced that by my discovery I have revealed to you more things than you were aware of."—"Gall, *in a familiar Letter to his Friend* Baron Retzer, 1798."

If we are to accept the verdict passed amidst mutual congratulations by the Physiologists of the period assembled at Bradford, we are on the eve of obtaining a revelation of the physiology of the brain by the localised application of electricity to its surface. Facts carefully observed and accurately recorded must always possess an intrinsic value, but it is possible to err in their interpretation; that this has been done to some extent with reference to the experiments in question, and exaggerated expectations founded on misconception indulged in as to the amount and accuracy of the knowledge to be expected from this source, is to me abundantly clear.

Enthusiasm in the pursuit of knowledge is doubtless amongst the highest of the characteristics which distinguish the noblest specimens of humanity from the common herd of mankind. As an evidence of mental activity, the jubilation with which the announcement of the results of applying electricity to the surface of the brain has been received is in the highest degree satisfactory. The more cordial the reception accorded these experiments, however, the more prominently the question obtrudes itself,—What are the distinctive differences in the path pursued to attain one common object by Fritsch, Hitzig, and Ferrier, and the method of Gall, that should occasion the results of the former to be welcomed with acclamation, whilst those of the latter were received with the hail of sneers, scoffs, ridicule, misrepresentation, and contumely? To the student of the human mind the difference, or rather contrast, offers a curious and interesting problem.

Can we find a partial explanation of the anomaly in the more purely physical character of the recent method of research—that the subject of attention in the one case is a movement visible to the senses, in the other a mental quality, an abstraction which presents no sensuous object to the mind? What is certain is, that many men have great taste and capacity for the observation, description, and arrangement of material facts, who are singularly deficient in the power of contemplating abstract existences. The majority of men appear to require a physical substratum for their thoughts. Their ideas are almost limited to *images*, or pictures of outward objects presented by the external senses; or secondly, to *conceptions* of actions being a change in the relation of material objects; or thirdly, to bodily sensations arising from the action of the external senses. Either the specialised senses—taste, smell, hearing, and sight—or the diffused sense of feeling, co-extensive with the surface of the body, and hence adopted as a generic term, and applied metaphorically (with its opposite poles, pleasure and pain) to all internal affections. They have not adequate power of abstraction to separate the subjective from the objective. Not analytical power sufficient to dig phantoms from their consciousness, isolate them from their surroundings, and hold them continuously before their mental vision for contemplation. They catch a glimpse of a figure for a moment, but before they have time to study its features it dissolves away like a wreath of mist. Now the subject matter of Phrenology is mental qualities, not material objects; whilst, in addition to its abstract basis, it superadds the doctrine of the dependence of the mental functions on certain external relationships of form and size, successfully to appreciate which demands an amount of preliminary study hardly likely to be expended on the problem, by those to whom one of the factors in the equation presents the aspect not merely of an unknown, but of an incommeasurable quantity. *Non omnia possumus omnes*, indeed, it is usually those whom some predominating instinct prevents being too discursive and keeps in one path of study by whom additions to the sum of human knowledge are made. Let us be thankful to the student, whose range of thought is limited to objects of sense, for his contributions to his own department; but do not let us regard him as an authority in others, nor commit the shallow blunder of citing his indifference to, or disbelief in, the invisible rays at the higher extremity of the spectrum as an argument for their non-existence. We have cultivators of the physical sciences, mathematicians, astronomers, natural philosophers, chemists in abundance, plenty of naturalists, ready to seize and describe all the peculiarities in form, size, weight, colour, distribution, and habits, of everything that has life. We have even a limited supply of metaphysicians and psychologists, who deal with *abstractions* and *words* in contradistinction to *things*, and inhabit an ideal world of their own. The dealers in things and the dealers in abstractions mostly dwell apart, and too often regard each others' pursuits with ill-disguised contempt.

Now the phrenologist requires to unite to a considerable extent the capacities and tastes of both classes; to combine the powers of mental analysis—the facility for detaining abstractions before the mind's eye for study—of the metaphysician and psychologist, with the instinct of observation and quick perception of physical differences by which the naturalist is distinguished—and in the fact that individuals who combine the two phases of capacity will be less numerous than those who possess one of the qualifications singly, we see an explanation of the cause why the scientific cultivators of phrenology are fewer in number than either the physicists or the metaphysicians.

In scanning the causes of the hostility Phrenology has so widely encountered, amongst others we must not omit to notice its close bearing on the personality of individuals. Men with little heads, little minds, but great vanity, rebel against a standard of capacity which gauges them correctly. A science which renders it possible—

> . . . . " A des signes certaines
> Reconnaitre le cœur des perfides humaines,"

will always have antagonists to whom such an idea is distasteful. The whole of the genus humbug, the empirics and impostors of the day, and men conscious of being at bottom thoroughly dishonest and unprincipled, instinctively recoil from a system which threatens to unmask their moral deformities to the eyes of the world, and reveal their true features, despite a whole wardrobe of trappings of duplicity. Napoleon boasted of having greatly contributed to put down Gall. His own medical attendent, Corvisart, one of the greatest physicians France ever produced, was an admirer of Gall, and vainly endeavoured to introduce him to the Emperor. "Corvisart," says Napoleon, "was a great partisan of Gall, and left no stone unturned (fit l'impossible) to push him on to me, but there was no sympathy between us." In short, Napoleon confessed he felt the greatest aversion for those "who taught that Nature revealed herself by external forms."

Again, the bulk of mankind have no doubt been organised by nature to lead a life of action, to do, and not to think. In youth they are plastic, and readily receive the impression stamped by their teachers, but by mature age the receptivity of childhood has vanished, and the clay of which they are composed refuses all attempts to mould it afresh; and especially is this the case where the egotistic feelings of self-love and vanity outweigh the pure love of knowledge for its own sake. Such men may indeed imbibe new ideas, and acquire an increase of knowledge as they grow older, but the new knowledge must have some points of affinity and harmony with the old, to be cordially welcomed. Above all, it must not threaten the subversion of those existing canons of belief which have hitherto guided them on life's journey, or it will infallibly excite antipathy and antagonism. Every day we have the spectacle of the direct testimony of facts being ignored and rejected without examination, from the inference that they are opposed to some cherished belief. Even the scientific *par excellence*, the professed philosophers, are not exempt from this human frailty; touch but the ark that enshrines the object of their worship, and you shall see the bigotry and intolerance with which they credit the theologian rivalled, if not outdone. As at the advent of Phrenology it encountered the antagonism of the religious world from its supposed tendency to materialism; so, at the present hour, many of our leading physicists shut their eyes to the curious phenomena of (the so-called) spiritualism, and open their mouths to assail its investigators, because they fear that these phenomena clash with that materialistic philosophy which constitutes the staple article of their scientific creed.

How vast a portion of our present stock of scientific knowledge would be non-existent if no one had been found to "take an interest" in the phenomena of magnetism! and can the most bigoted apostle of the new positive-physical gospel venture to assert that a domain of fact as wide in its extent and fruitful in its result may not lie hidden, awaiting conquest by man in this force of source unknown, the conditions attending the presence of which, though yet undiscovered, we may be assured are governed by laws as definite and immutable as those of gravitation. We do not yet know how to multiply mediums at pleasure, as we do magnets, because we know neither the species of loadstone nor the kind of manipulation required, but all honour to those who are engaged in the research.

Apparently as long as psychologists were content to frame theories out of their own consciousness, and confined themselves to abstractions, their researches created no antagonism in the physicists who occupied themselves with the study of material objects and their properties and functions; but when these saw their own peculiar province invaded, and the physiology of the highest organ of the body, to them an enigma, for the solution of which neither their tastes nor capacities were adapted, declared to be unravelled by a method of study for which they had no proclivity, and by an individual who had altogether surpassed them in their own province of anatomy, their pride rebelled, and their wounded *amour propre* found vent in denunciations as outrageous and absurd as ever greeted the author of a new discovery. English metaphysicians, and immaterialist divines also, led by English anatomical authorities to regard the propounder of these new doctrines as an ignorant quack, were not slow in joining the chorus of detraction and abuse against the audacious innovator, who overthrew all their cherished theories as to the independence of the mind on organisation—the former viewing the doctrines of Gall with profound contempt and disgust as tending to degrade man to the level of brutes, the latter with repugnance and alarm as threatening to sap the foundations of religion.

Dr. John Gordon, a lecturer on anatomy of great reputation in Edinburgh, in an article in the "Edinburgh Review," in 1815, said, "We look upon the whole doctrines taught by these two modern peripatetics (Drs. Gall and Spurzheim), anatomical, physiological, and physiognomical, as a piece of thorough quackery from beginning to end." Lord Jeffrey, in the same periodical, in 1826, designated the doctrines as "crude," "shallow," "puerile," "fantastic," "dull," "dogmatic," "incredibly absurd," "foolish," "extravagant," and "trash." The "Quarterly Review," in their notice of Madame de Stael's "L'Allemagne," censured her for being "by far too indulgent to such ignorant and interested quacks as the craniologist Dr. Gall," and in No. XXV. the same Review declared the new science to be "sheer nonsense," and designated Dr. Spurzheim as "a fool." The Rev. Thomas Rennell, Christian advocate at Cambridge, in his "Remarks on Scepticism, especially as it is connected with the subjects Organisation and Life," assures his readers that the system of Gall and Spurzheim "is annihilated by the commonest reference to fact," spoke of "its absurdities," of this "masterpiece of empiricism," and designated it as "the flimsy theories of these German illuminati." Whilst as late as 1836, Sir CharlesBell wrote—"The most extravagant departure from all the legitimate modes of reasoning, although still under the colour of anatomical investigation, is the system of Dr. Gall. Without comprehending the grand divisions of the nervous system, without a notion of the distinct properties of the individual nerves, or having made any distinction of the columns of the spinal marrow, without having even ascertained the difference of cerebrum and cerebellum, Gall proceeded to describe the brain as composed of many particular and independent organs, and to assign to each the residence of some special faculty."

The insular ignorance of Gall's anatomical discoveries, position in the scientific world, and true character displayed in these insulting criticisms, is no less disgraceful than astounding. Professor Hufeland, an anatomist and physiologist of European reputation, thus expresses himself concerning Gall:—"It is with great pleasure and much interest that I have heard this estimable man himself expound his new doctrine. I am fully convinced that he ought to be regarded as one of the most remarkable phenomena of the 18th century, and that his doctrine should be considered as forming one of the boldest and most important steps in the study of the kingdom of nature. One must see and hear him to learn to appreciate a man completely exempt from prejudices, from charlatanism, from deception, and from metaphysical reveries. Gifted with a rare spirit of observation, with great penetration and a sound judgment, identified, as it were, with nature, he has collected a multitude of signs of phenomena which nobody had remarked till now—has discovered the relations which establish analogy between them—has learnt their signification—has drawn consequences and established truths, which are so much the more valuable that, being based on experience, they emanate from nature herself."

"The worthy Reil," says Professor Bischoff, "who as a profound anatomist and judicious physiologist stands in no need of my commendation, has declared, in rising above all the littleness of egotism, that he had found more in the dissections of the brain performed by Gall than he had conceived it possible for a man to discover in his whole life-time!"

"Loder," continues Professor Bischoff, "who certainly does not yield the palm to any living anatomist, has expressed the following opinion of the discoveries of Gall:—'The discoveries of Gall in the anatomy of the brain are of the highest importance, and many of them possess such a degree of evidence that I cannot conceive how any one with good eyes can mistake them. I refer to the great ganglion of the brain—to the passage of the corpora pyramidalia into the *crura* of the brain and the hemispheres—to the *fasciculi* of the spinal marrow—to the crossing of the fibres under the pyramidal and olivary eminences—to the recurrent fibres of the cerebellum—to the commissures of the nerves—

to the origin of the motor nerves of the eyes, of the trijeminal nerves, of those of the sixth pair, &c. These discoveries alone would be sufficient to render the name of Gall immortal; they are the most important which have been made in anatomy since the discovery of the system of the absorbent vessels. The unfolding of the brain is an excellent thing. What have we not to expect from it as well as from the ulterior discoveries to which it opens the way? I am ashamed and angry with myself for having, like the rest, during thirty years, sliced down hundreds of brains as we cut a cheese, and *for having missed seeing the forest on account of the great number of trees it contained*. But it serves no purpose to distress one's self, and to be ashamed. The better way is to lend an ear to truth, and to learn what we do not know.'"

Which latter piece of advice I commend to the notice of those little great men, the eminent compilers, who,—devoid of the original genius which, by perceiving relationships before unknown and unsuspected, confers new principles on science,—would fain set themselves up as physiological authorities on the strength of their book-making capacities.

Not only were the great and important additions made by Gall and Spurzheim to the anatomy of the nervous system fully admitted by Cuvier, but their position as the highest authorities on the subject was so fully recognised in Paris, in 1813, that the article, "Anatomie du Cerveau," for the "Dictionnaire des Sciences Medicales," was confided to their care.* All English anatomists, however, have not followed the suit of Dr. John Gordon and Sir Charles Bell, in recording at once their jealousy and their ignorance by absurd denunciation of Gall. Mr. Grainger, the greatest English authority of his day on the anatomy of the brain and spinal cord, writes, "The true anatomy of the cerebrum was perfectly unknown till the researches of Gall, and it is due to the character of this eminent man and of his pupil, Spurzheim, to state that all our knowledge of the anatomy of both the brain and spinal cord has resulted from their inspections;" and Joshua Brookes, in his lectures, and Mr. Solly, in his well-known work on the anatomy of the brain, have done full justice to the anatomical discoveries of Gall.

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* The necessary result of the old method of dissecting the brain is thus pithily described in this article:—"On a mis en usage une méthode de dissection très-défectueuse; on ne fesait que des coupes horizontales, verticales, ou oblique, par en haut ou par en bas et on enlevait successivement des tranches de cet organe. De cet manière, *on commencait par détruire les connexions des différens appareils et on procédait sans égard pour l'ordre dans lequel les parties se suivent naturallement*."

The method pursued by Gall, in seeking to ascertain the functions of the brain, was by comparing the power of manifesting particular mental faculties with the size and condition of particular portions of this organ. Phrenologists believe this method to be vastly superior to all others, and, in justification of this opinion, point to the rich harvest it has produced in contrast to the barren results which have hitherto been obtained by the employment of mutilations and the application of stimuli. Is there, at the present moment, a single physiologist in a position to declare that, after qualifying himself to judge of the development of the organs by the requisite study, the result of careful examination has convinced him that the localities assigned by Gall to the primitive mental faculties are erroneous? Why is this sound and legitimate mode of studying the functions of the brain neglected and ignored by physiologists in general, "who seem desirous of exhausting every possible variety of error before they will adopt it?" Men of science are usually eager to avail themselves of every practicable means in the pursuit of knowledge, but it would appear to be a desideratum to discover the functions of the brain by other than phrenological methods.

In addition to employing mutilations, Rolando trephined the cranium of various quadrupeds, and applied one of the poles of a voltaic pile to different portions of the brain, whilst the other was applied to different parts of the body. With reference to these experiments of Rolando, and the experiments by mutilation of Flourens, Gall remarked:—

"It is a subject of constant observation that, in order to discover the functions of the different parts of the body, anatomists and physiologists have always been rather disposed to employ manual means than to accumulate a great number of physiological and pathological facts,—to combine these facts, to reiterate them, or to await their repetition in case of need,—and to draw slowly and successively the proper consequence from them, and not to announce their discoveries but with a wise reserve. This method, at present the favourite one with our investigating physiologists, is imposing from its materiality; and it gains the approbation of most men by its promptitude and its apparent results. But it has also been constantly observed that what has appeared to have been incontestably proved by the mutilator A., either did not succeed with the mutilator B., or that he had partly found in the same experiments all the proofs necessary to refute the conclusions of his predecessor. It is but too notorious that similar violent experiments have become the scandal of the Academicians, who, seduced by the attraction of ingenious operations, have applauded with as much enthusiasm as fickleness the pretended glorious discoveries of their candidates.

"In order that experiments of this kind should be able to throw light on the functions of each of the cerebral parts, it would require a concurrence of many conditions impossible to be fulfilled. It would first require that we should be enabled to restrain all the effects of the lesion to that portion only on which the experiment is performed; for if excitement, hæmorrhage, inflammation, &c., affect other parts, what can we conclude? and how can we prevent these inconveniences in mutilations either artificial or accidental? It would be necessary that we should be able to make an animal whose brain has been wounded and mutilated—who is filled with fear and suffering, disposed to manifest the instincts, propensities, and faculties, the organs of which could not have been injured or destroyed. But captivity alone is sufficient to stifle the instincts of most animals."

Have the results attained by the recent experiments of Fritsch, Hitzig, and Ferrier a tendency to invalidate these opinions of Gall, or do they not rather confirm their correctness? I presume it will hardly be pretended that the function of a single portion of the brain has yet been discovered by these means,* and I venture to think there is but little probability of their effecting such a discovery in the future, notwithstanding the exaggerated expectations held out. At present it is palpable that physiologists are quite adrift as to the real signification of the phenomena elicited, the true interpretation of which must be sought in the discoveries of Gall, who maintained the competency of the surface of the brain to originate muscular movements in opposition to the current doctrines of physiology and the asserted proof to the contrary afforded by the experiments of Flourens, and other mutilators, and whose familiarity with the fact is recorded in the extract from his letter to Baron Retzer, in 1798, prefixed to this article.

The explanation of the phenomena obtained by the application of stimuli to the surface of the brain, is found in the fact that those innate faculties which require the aid of the muscular system to carry out their behests have the power of originating the movements necessary for this purpose; and hence, when Dr. Ferrier applied a galvanic current to the cortical surfaces of the organs of the instinct "to take food," "to seize prey," "to destroy," "to fight," "to construct,"—movements "of mastication," "of striking with the claws, or seizing with the mouth," "of biting and worrying," "of scraping, or digging," ensued: whilst the stimulation of the same locality (constructiveness) which put the fore paws and hind legs in action in the rabbit, would, in the beaver, superadd the motion of the incisor teeth and the tail. What can be more palpable than that the inferences to be obtained from such experiments are not only far more vague and indefinite than those furnished by the employment of the phrenological method, but absolutely incapable of ascertaining the shape, and defining the boundaries, of the organs, as has been accomplished by Gall in the case of locality, the shape of which he ascertained to be similar in dogs to its form in man. In short, little more can be said on behalf of these experiments at present than that in a cloudy and obscure form they lend a vague general confirmation (not required) to the correctness of the localities assigned to the primitive faculties by phrenologists.

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* A fact conclusive on this point, and which places in a striking light the vagueness and want of precision of the results obtained, is the circumstance that that eminent compiler, Dr. Carpenter, sees in these experiments "a remarkable confirmation" of his transcendently absurd and ridiculous notion that the intellectual organs are seated in the back of the head.

Amongst the many eminent men whose researches and discoveries have shed honour on the profession of medicine, Gall will assuredly by posterity be accorded a place second to none. Man had looked on man, and scanned the face of his brother in sunshine and in storm, in friendship and in anger, for countless thousands of years, without having succeeded in seizing and individualising a single primitive faculty, much less in discovering its seat. The advent of Gall broke up the long night of darkness and error as to their own being, under which the human race had slumbered for ages. Sensation, perception, memory, judgment, imagination—the idolæ of the past—the stock properties of every psychological system from that of Aristotle downwards, instead of being primitive faculties, were clearly demonstrated by the most masterly analysis and the most unanswerable arguments to be simply different degrees or consecutive modes of action proper to each of the elementary intellectual faculties, and necessarily variable in strength in relation to subjects specifically distinct. Gall studied the maximum or minimum exhibition of certain passions or capacities compared with the extreme or defective development of certain parts of the brain; and when a vast number of concurrent experiences had satisfied him of a connection, named the primitive faculty by the simplest words indicative of its function to be found in the vocabulary of every-day life. He thus replaced the phantoms of the metaphysicians, which explained nothing, by terms which speedily asserted their vitality by being constantly heard in the mouths of the people to assist them in defining and describing their fellow-men, thus at once obtaining that sanction from the spontaneous dictates of popular common-sense, which is the surest test of the truth of all fundamental ideas.

It is a common doctrine that discoveries are seldom made by an individual greatly in advance of the scientific mind of the day, or without other investigators having been placed by the existing state of knowledge on the same track as the more fortunate discoverer, who is thus merely credited with having by a short date anticipated other investigators in bestowing a new fact, or idea, on mankind. With regard to the discovery of phrenology, however, made at the close of the last century by Dr. Gall, if we may judge by the fact that what he discovered the great mass of his contemporaries never succeeded in recognising, even when the locality for research was pointed out to them, and the means of observation lay in profusion everywhere around, there appears every reason to believe, that but for the appearance of a man of his rare and exceptional genius, the vast contribution to human knowledge for which the world is indebted to his labours would still have been slumbering in the womb of futurity. I venture to assert that no body of doctrines were ever established on a series of observations more cautiously conducted, rigorously scrutinised, and patiently verified than those of phrenology by Dr. Gall, who devoted his entire life and all his pecuniary resources to this object, finding his reward in the consciousness of the importance of his discoveries, and that prophetic vision of the future which placed him above contemporary jealousies, and caused him to exclaim in calm self-reliance, "This is *truth*, though opposed to the philosophy of ages!"

"I have always," says Gall, "had a consciousness of the dignity of my researches, and of the extended influence which my doctrine will hereafter exercise on all the branches of human knowledge, and for this reason I remain indifferent to all that may be said either for or against my works. They differed too much from the received ideas of the times to be appreciated and approved at first. . . . . My views of the qualities and faculties of man are not the fruit of subtle reasonings. They bear not the impress of the age in which they originate, and will not wear out with it. They are the result of numberless observations, and will be immutable and eternal like the facts that have been observed and the fundamental powers which these facts force us to admit."

VII. ECONOMY OF FUEL.

By Frederick Charles Danvers, Assoc. Inst. C.E.

THE important question of fuel economy is one which has, within the last few years, attracted the attention of several of our learned societies, and it is one which cannot fail to grow in importance rather than diminish. Practically speaking, coal is the only fuel upon which we can with any certainty rely for our great manufacturing industries; and although this may, in some measure, be supplemented by a more extensive use of peat than has hitherto been the case, still coal must continue to be looked upon as the great staple upon which the chief of our manufactures must continue to depend. Coal, as we have shown upon several former occasions, exists only to a limited extent, and our existing resources of that fuel are not capable of being reproduced, as is the case with wood, and to some extent also with peat; and, in order to make our coal supply keep pace with the ever increasing demand upon it, one of the most important considerations now is, how so to economise its use in all branches of manufacture and for steam and domestic purposes as to minimise the amount of waste which at the present time takes place in the use of it. With the view, therefore, of pointing out what may be done in that direction, we purpose now to consider how far scientific and mechanical improvements have already succeeded in that direction, and to what extent further economies may be possible.

Each pound of coal possesses a certain number of heat units, and to produce any given results from its combustion requires the development of a given number of heat units; when, therefore, we find that in order to produce such results a considerable number of heat units are expended in excess of the theoretical requirements, it is very evident that a certain amount of avoidable waste is taking place. It is not to be expected that the full economic value of coal can ever be attained in most cases, for were such the case it would necessarily follow that the escaping volumes from combustion must only be permitted to escape when all the heat given out in combustion has been effectually abstracted, and all radiated heat would also require to be reserved for useful purposes.

There can be no doubt that coal burnt under a certain amount of compression will yield the best results; but the important question is how to regulate the admission of air so that no more is employed in supporting combustion than is actually required for that purpose; any excess above that amount must tend only to lower the temperature, and so abstract a certain amount of useful effect from the fuel. The air admitted for this purpose must also be so regulated as to ensure its complete combination with the fuel, so that no air in a free state shall pass away undecomposed. The channel for escaping vapours should also be so regulated in size as to give no more than sufficient space for their free passage in a certain state of expansion, whilst the current of air into the furnace must also be maintained at a rate sufficient for the combustion of the proper amount of fuel for the work to be effected. Now, in order to maintain this state of things, it is necessary to produce an upward current in the chimney, and in order to do this, the ascending vapours must either be forced out by mechanical means, or they must be allowed to escape at a temperature above that of the atmosphere. In either case, therefore, it is clear that a certain amount of heat must be developed in excess of what would otherwise be required for the purpose. To obtain, therefore, the full theoretic value of the coal burnt for any specific purpose must be, under these considerations, absolutely impracticable; the nearer we attain to that point, however, the less cause of complaint there will be of a waste of coal taking place in any particular case. As we shall presently see, the use of hot-blast in iron smelting was followed by a decided saving in the amount of fuel required to produce 1 ton of pig-iron, notwithstanding that a certain portion of it was consumed in first heating the air for the blast. It will readily be understood how the introduction of cold air into any furnace must have the immediate effect of lowering the temperature, and it is found that the amount of fuel necessary to heat the air before admitting it into the furnace is less than the amount required to maintain the temperature within the furnace when it is fed with cold air. A free current for escape of the vapours of combustion is followed by the escape of the more volatile portions of the fuel used, which are therefore absolutely lost; whilst, with a strong blast, where there is a free escape—as in a locomotive—small particles of fuel, wholly uncarbonised, are also carried away without doing any effective work. Although, in both these cases, the greater portion of the fuel remains behind until it is wholly consumed, only a small proportion of its effective value is retained, whilst the greater part is lost, representing so much waste of fuel, a great portion of which must be looked upon as avoidable loss.

To turn now from generalities to the practical working of the question, we shall proceed to consider first the application of these principles in the manufacture of iron, which branch of industry alone consumes about one-third of the total amount of coal raised in the United Kingdom, and which, therefore, most largely affects the great coal question of the present day.

Taking the results of iron manufacture in Scotland, we find, upon the authority of Dr. Percy, that the ton of pig-iron, as made in 1829 at the Clyde Iron Works, required the coke of 8 tons 1¼ cwts. of coal; whilst in the following year, owing to Neilson's invention of the hot-blast to iron furnaces, the introduction of air heated to 300° F. brought down the consumption per ton of pig to 5 tons 3¼ cwts. 8 cwts. of coal were consumed in heating the blast, so that the actual saving per ton of pig-iron was 2½ tons. In 1833, when raw coal had come to be used instead of coke, 1 ton of pig-iron was made with 2 tons 5½ cwts. of coal, which, with 8 cwts. for heating the blast, made a total of 2 tons 13½ cwts. Hence by the application of the hot-blast, the same amount of fuel reduced three times as much iron, and the same amount of blast did twice as much work as previously.

Subsequently to the attainment of the foregoing results an increase in the size of the blast-furnace has been followed by still further economy in fuel used in the manufacture of iron. The discovery of this fact is due to the iron smelters of Cleveland. When the first blast-furnace was erected in that district by Mr. John Vaughan, in 1851, the practice of older districts was followed, and the furnace was made 42 feet high by 15 feet diameter at the bosh. Up to 1858 there was a gradual increase of size, the furnaces that year being 56 feet in height by 16 feet bosh. The results of this increase of size were so satisfactory that Mr. Vaughan was led to rebuild one of the old furnaces, increasing its size to 61 feet high by 16 feet 4 inches bosh. This may be said to be the first decided step towards the great increase in size which followed, the comparative results being so much in favour of the large furnace over the original small one that it soon became an undoubted fact that economy was to be found in that direction. Although the scientific reasons which led to a saving of fuel through an increase in size were at that time not clearly understood, yet the practical results obtained were so beneficial that they culminated in a revolution unparalleled in the blast-furnace history of any district, in which all the original furnaces and plant were razed to the ground, and new ones on the now established improved principles were built in their stead. Furnaces have been built over 100 feet in height, and some of them as wide as 30 feet in the bosh; and it is the opinion of those best competent to judge of the matter, that the useful maximum of both height and diameter have been attained, if not exceeded. The object of increasing the size of the Cleveland furnaces was twofold; first, to increase the make; and, secondly, to economise fuel; a third has followed gratuitously, viz., improvement in quality. The saving of coke from this cause has been considerable, and may be put down at from 7 to 8 cwts. of coke per ton of iron made. Mr. Isaac L. Bell, in investigating the causes which led to this economy of fuel in the larger furnaces, discovered that in a furnace one or two combinations are possible—that of one equivalent of carbon uniting with one or with two equivalents of oxygen; and that in the latter case as much heat is developed by 20 cwts. as is done by 71·14 cwts. when the carbon only unites with one equivalent of oxygen.

The cause of the saving in fuel in large furnaces is twofold; first, by the interception of a considerable portion of the heat formerly carried away in the gases—the products of combustion of the old furnaces; and, secondly owing to a better state of oxidation or combustion of the carbon,—a state of things proved by a great number of chemical analyses of the gases themselves as they leave the furnaces. Mr. Bell has also proved that no subsequent additions to the size of the furnace, beyond a certain point, has been attended with anything like the saving which accompanied the first steps in that direction; and that this is due to the fact that the escaping gases have, by such increase in dimensions, been deprived of nearly all the heat they can be made to surrender for use in the furnace; and that the chemical action in a furnace of about 12,000 feet is as perfect, so far as numerous analyses of the gases could indicate, as it is in a furnace of 25,000 cubic feet.

Next to the increased size of the furnace, the principal cause of economy in fuel in the manufacture of iron is the improved temperature of blast at the tuyeres, which has been increased up to 1400°. In order to obtain this temperature, heating-stoves are employed, consisting most generally of a series of iron pipes within a furnace, through which the heat for the blast is drawn. The highest temperature and the best results have, however, been obtained with Whitwell's hot-blast fire-brick stoves, by the use of which, at Consett, iron has been made with 17 cwts. 2 qrs. of coke per ton, the blast being at a pressure of 3 lbs. per inch, and the temperature about 1400°. With an increased pressure of blast to 4 lbs., and a decrease in temperature to 1200°, an increased production of pig-iron was the result, but the consumption of coke rose to 19 cwts. 2 qrs. per ton.

Under the most economical system of working, with open-topped furnaces, an enormous amount of fuel is wasted by the escape of the vapours of combustion, many of them only half consumed at a high temperature. So early as the beginning of the present century (1811), the important practical problem of the utilisation of the waste gas of iron-smelting furnaces was solved in a satisfactory manner in France; but upwards of five and twenty years elapsed before it began to attract the serious attention of iron-masters in Great Britain or on the Continent of Europe. The first attempts made in this country were exceedingly crude, and much of the carbonic oxide was allowed to escape unburnt into the air, by which an enormous amount of heat, capable of being developed by the combustion of that gas, was lost. The calorific effect of the waste gas is due partly to its sensible heat, and partly to the heat developed by its combustion in contact with atmospheric air. In some furnaces, the gas is taken off through several circular openings at a short distance below the level of the solid contents of the furnace, their exhaustion being effected by means of a high stack. In others, there is an annular passage or flue near the mouth, extending all round, and communicating with the interior by several short passages, and in this case, also, the aid of a stack is required for exhaustion. The most general method is, however, to close the top of the furnace with a "cup and cone."

We must not close our account of economy of fuel in the blast-furnace without some reference to Ferrie's self-coking blast-furnace. In considering the problem of utilising the gases escaping from blast-furnaces worked with raw coal, it occurred to Mr. Ferrie that much of the difficulty would be overcome if the coal could be coked in the furnace in somewhat the same way as it is coked in gas retorts. In the application of these ideas to a large furnace at the Monkland Works, the mouth of the furnace is closed by a bell and hopper, and the gases are led off to the blast-heating stoves in the usual way. The upper part of the furnace, for a depth of 20 feet below the space required for the bell and cone, is divided into four compartments by vertical walls supported on arches, and radiating from the centre. These division walls, by causing additional frictional resistance to be opposed to the descent of the materials, relieve the coke formed of a portion of its load, but their main object is to enable the coking of the coal to be performed in the upper part of the furnace. The economic results obtained with this furnace have been most satisfactory, and they were thus described by Mr. Ferrie himself to the Iron and Steel Institute, at their meeting in March, 1871:—

"In the Lanarkshire district, the quantity of coal required in the manufacture of a ton of No. 1 pig-iron ranges from 50 to 52 cwts. in the furnace, whereas, in this furnace, a ton of the same quality can be produced with 32 to 36 cwts., effecting a saving in coal of nearly a ton to the ton of iron made. In ores, the saving in this furnace will be about 2½ cwts. per ton of iron."

The quantity of gas drawn off from the furnace is found to be greatly in excess of that required for heating the blast and raising steam for the blowing engines; but where works for the production of finished iron are annexed to the blast-furnaces, ready means may be found for utilising this excess of gas.

The next subject for consideration is the economy of fuel hitherto attained in the manufacture of iron and steel. Before the introduction of the puddling process, the conversion of cast-iron into malleable or wrought-iron was always effected in a finery or hearth, in which the metal was melted in contact with the solid fuel, and so exposed to the highly oxidising action of a blast of atmospheric air. Dr. Siemens, in a lecture delivered to the operative classes of Bradford, on behalf of the British Association, in September last, remarked that in the metallurgical furnace there is great room for improvement, the actual fuel consumed in heating a ton of iron up to the welding point, or in melting a ton of steel, being more in excess of the theoretical quantity required for those purposes than is the case with regard to the production of steam power or to domestic consumption. Taking the specific heat of iron at 114, and the welding heat at 2700 degrees F., it would require 307 heat units to heat 1 lb. of iron. A pound of pure carbon develops 14,500 units of heat, a pound of common coal 12,000, and therefore 1 ton of coal should bring 39 tons of iron up to the welding point. In an ordinary re-heating furnace, a ton of coal heats only 1⅔ tons of iron, and therefore produces only one twenty-third part of the maximum theoretic effect. In melting one ton of steel in pots, 2½ tons of coke are consumed; and, taking the melting point of steel at 3600° F., the specific heat at 0·119, it takes 428 heat units to melt a pound of steel; and, taking the heat-producing power of common coke also at 12,000 units, 1 ton of coke ought to be able to melt 28 tons of steel. The Sheffield pot steel melting-furnace, therefore, only utilises one-seventieth part of the theoretical heat developed in the combustion. Several methods are now in use whereby greater economy in fuel results, but we shall not now do more than give special notice to one of these, as the object of the present article is not so much to refer to all methods of economising fuel, but rather to point out to what extent economy has been attained in various branches of consumption. We propose here merely to specify the Siemens's regenerative furnace, which is now too well known to require any detailed description, and by the use of which a ton of steel is melted with 12 cwts. of small coal, whereas in the ordinary furnace at Sheffield, about 3 tons of Durham coke are necessary to accomplish the same end. In this one operation, therefore, in the process of steel manufacture, a saving of four-fifths of the fuel ordinarily employed is capable of being effected.

Turning now to Mr. Bessemer's system of steel manufacture, we find where 3½ tons of coke are ordinarily used per ton of steel, 3 cwts. only is required for his process; and we find that gentleman stating, in evidence, before the Coal Commission of 1871, that "if we take the present production of cast steel, in this country, by my process, at 150,000 tons a year, we should have a saving of a little over half a million tons of coke in that time, representing, of course, its proportion of coal, the amount being greater or less according to the purity of the coal employed."

In estimating the waste of fuel under steam boilers, it is necessary to remember that in burning 1 lb. of carbon in the presence of free oxygen, carbonic acid is produced, and 14,500 units of heat are liberated. Each unit of heat is convertible into 774 units of force or mechanical energy; and hence, 1 lb. of carbon represents really 11,223,000 units of potential energy; that is to say, the mechanical energy set free in the combustion of 1 lb. of pure carbon is the same that would be required to raise 11,223,000 pounds weight one foot high, or as would sustain the work which we call a horse power during five hours thirty-three minutes. Practically, however, the results obtained fall very far short of these results. An ordinary non-expansive non-condensing engine requires commonly a consumption of from 10 lbs. to 12 lbs. of coal per horse-power per hour, whereas a good expansive and condensing engine accomplishes the same amount of work with 2 lbs. of coal per hour. In order to attain the greatest economy of fuel, used for the purpose of producing mechanical action, it is first necessary to provide such an amount of heating surface in the boiler as shall absorb all the heat produced by combustion, and transmit it to the water. The beneficial results which are attained by the greater size of boiler in relation to the coal burnt and to the horse-power required have been proved by actual usage, and are not merely matters of calculation. The Institute of Mechanical Engineers instituted a careful inquiry, in 1863, into the consumption by the best engines in the Atlantic Steam Service, and the result showed that it fell in no case below 4½ lbs. per indicated horse-power per hour. Last year they assembled with the same object in view in Liverpool, and Mr. Bramwell produced a table, showing that the average consumption by seventeen good examples of compound expansive engines did not exceed 2¼ lbs. per indicated horse-power per hour. Mr. E. A. Cowper has proved a consumption not exceeding 1½ lbs. per indicated horse-power per hour, in a compound marine engine constructed with an intermediate superheating vessel in accordance with his plans. Dr. Siemens has, however, proved that theoretical perfection would only be attained if an indicated horse-power were produced with about ¼ lb. of ordinary steam coal per hour.

Mr. Bramwell, in his address as President of Section G, at the meeting of the British Association at Brighton, in 1872, bore testimony to the fair duty done by locomotive engines, which he stated to be due, first, to the fact that, since the introduction of coal the furnaces have been to a considerable extent gas furnaces, with a free admission of air through open fire doors to the surface of the fuel; and, secondly, to the fact that the boilers have large absorbing powers. In marine engines there has, within the last ten years, been an enormous saving. The old fashioned engine, working at 20 lb. steam and with injector condensers, is being abandoned for engines generally on the compound cylinder principle, working at 60 lb. and 70 lb. steam, highly expansive, and fitted with surface condensers; and the result is a reduction of the consumption of fuel in the same vessels, on the same voyages, and performed in the same time, of from 40 to 50 per cent of that which was previously burnt.

Irrespective, however, of other circumstances, a great waste of fuel may be caused by bad firing; the fire being kept too thick, or too thin, or irregular. If too thick, the carbonic acid that is generated by the combustion of the lower part of the fuel, with which the air first comes in contact, is changed in its passage through the upper part of the fuel into carbonic oxide, by absorbing from the fuel a second equivalent of carbon. If carbonic oxide gas, thus generated, does not meet with free atmospheric air, at a suitable temperature in the upper part of the furnace, it must remain unconsumed, and will pass through the flues or tubes of the boiler, and make its escape into the air, carrying with it the valuable unconsumed carbon of the coal in a gaseous form. And when it is remembered that under ordinary circumstances every pound of coal burnt into carbonic acid is capable of evaporating about 13 lbs. of water from 212°, while a pound of coal converted into carbonic oxide is capable of evaporating only 4 lbs. of water, it will be seen how necessary it is that no mismanagement of the fire should cause a portion of the fuel thus to escape unburnt up the chimney.

We have thus pointed out the extent to which, and the principal means by which economy of fuel has been attained in the manufacture of iron, and for steam purposes. The results hitherto attained are, however, still very far from what theoretically should be practicable; but there can be little doubt that the same influences which have been at work to produce established results, will continue to act in the same direction, and with equally beneficial effects, until very much better value is obtained out of coal, although it would be unreasonable to hope that the full theoretical value should ever be actually attained; and, indeed, as has been already shown, such a result would be impossible.

Lastly, a few words with reference to the waste of fuel in domestic consumption. Nothing could be more extravagantly absurd, from an economical point of view, than the present system of open fire-places, and the method of setting them. From published returns for the year 1872 it appears that, taking the statistics of the metropolitan district as a guide, on an average, 12¾ cwts. of coal is consumed per annum for each person of the population for domestic services. In the preceding year the average was slightly higher, but prior to that it was below that average; so that it appears, in the use of fuel for domestic purposes, so far from there having been any attempt at economy, the reverse has been the case, and this increasing extravagance has only been temporarily checked by recent high prices.

The common practice in house building is to put the fire-grate immediately below and within a chimney; and, as this chimney is formed of brickwork, by no possibility can more than the most minute amount of heat be communicated from the chimney to the room. The main part of the conducted heat of the fire inevitably goes up the chimney, and is wasted, leaving the room to be warmed principally, if not entirely, by the radiated heat. Besides this, it must be remembered that, ordinarily speaking, no provision is made by architects or builders for the proper supply of air to the fire-places, and hence arise smoky chimneys and other evils of the present system. Here, then, is room for much improvement, and we are glad to perceive that the Society of Arts is giving its attention seriously to the matter, and it is to be hoped that some beneficial effects may be the result. As an evidence of how improved efficiency may be combined with economy, in this respect we may refer to Captain Douglas Galton's fire-grate, on which a paper was read before the British Association at Norwich, in 1868. This consists in putting a flue to the upper part of the fire-grate, which flue passes through a brick chamber formed in the ordinary chimney; this chamber being supplied with air from the exterior of the room by a proper channel, and then the air, after being heated in contact with the flue in the chamber, escapes into the room by openings near the ceiling, so that the room is supplied with a copious volume of warm fresh air, thus doing away with all tendency to draughts from the doors and windows, and furnishing an ample supply for the purposes of ventilation and combustion.

VIII. NOTES OF AN ENQUIRY INTO THE PHENOMENA CALLED SPIRITUAL,

DURING THE YEARS 1870-73.

By William Crookes, F.R.S., &c.

LIKE a traveller exploring some distant country, the wonders of which have hitherto been known only through reports and rumours of a vague or distorted character, so for four years have I been occupied in pushing an enquiry into a territory of natural knowledge which offers almost virgin soil to a scientific man. As the traveller sees in the natural phenomena he may witness the action of forces governed by natural laws, where others see only the capricious intervention of offended gods, so have I endeavoured to trace the operation of natural laws and forces, where others have seen only the agency of supernatural beings, owning no laws, and obeying no force but their own free will. As the traveller in his wanderings is entirely dependent on the goodwill and friendliness of the chiefs and the medicine men of the tribes amongst whom he sojourns, so have I not only been aided in my enquiry in a marked degree by some of those who possess the peculiar powers I have sought to examine, but have also formed firm and valued friendships amongst many of the recognised leaders of opinion, whose hospitalities I have shared. As the traveller sometimes sends home, when opportunity offers, a brief record of progress, which record, being necessarily isolated from all that has led up to it, is often received with disbelief or ridicule, so have I on two occasions selected and published what seemed to be a few striking and definite *facts*; but having omitted to describe the preliminary stages necessary to lead the public mind up to an appreciation of the phenomena and to show how they fitted into other observed facts, they were also met, not only with incredulity, but with no little abuse. And, lastly, as the traveller, when his exploration is finished and he returns to his old associates, collects together all his scattered notes, tabulates them, and puts them in order ready to be given to the world as a connected narrative, so have I, on reaching this stage of the enquiry, arranged and put together all my disconnected observations ready to place before the public in the form of a volume.

The phenomena I am prepared to attest are so extraordinary and so directly oppose the most firmly rooted articles of scientific belief—amongst others, the ubiquity and invariable action of the law of gravitation—that, even now, on recalling the details of what I witnessed, there is an antagonism in my mind between *reason*, which pronounces it to be scientifically impossible, and the consciousness that my senses, both of touch and sight,—and these corroborated, as they were, by the senses of all who were present,—are not lying witnesses when they testify against my preconceptions.*

But the supposition that there is a sort of mania or delusion which suddenly attacks a whole roomful of intelligent persons who are quite sane elsewhere, and that they all concur to the minutest particulars, in the details of the occurrences of which they suppose themselves to be witnesses, seems to my mind more incredible than even the facts they attest.

The subject is far more difficult and extensive than it appears. Four years ago I intended only to devote a leisure month or two to ascertain whether certain marvellous occurrences I had heard about would stand the test of close scrutiny. Having, however, soon arrived at the same conclusion as, I may say, every impartial enquirer, that there was "something in it," I could not, as a student of nature's laws, refuse to follow the enquiry wheresoever the facts might lead. Thus a few months have grown into a few years, and were my time at my own disposal it would probably extend still longer. But other matters of scientific and practical interest demand my present attention; and, inasmuch as I cannot afford the time requisite to follow the enquiry as it deserves, and as I am fully confident it will be studied by scientific men a few years hence, and as my opportunities are not now as good as they were some time ago, when Mr. D. D. Home was in good health, and Miss

* The following remarks are so appropriate that I cannot forbear quoting them. They occur in a private letter from an old friend, to whom I had sent an account of some of these occurrences. The high position which he holds in the scientific world renders doubly valuable any opinion he expresses on the mental tendencies of scientific men. "Any *intellectual* reply to your facts I cannot see. Yet it is a curious fact that even I, with all my tendency and desire to believe spiritualistically, and with all my faith in your power of observing and your thorough truthfulness, feel as if I wanted to see for myself; and it is quite painful to me to think how much more proof I want. Painful, I say, because I see that it is not reason which convinces a man, unless a fact is repeated so frequently that the impression becomes like a habit of mind, an old acquaintance, a thing known so long that it cannot be doubted. This is a curious phase of man's mind, and it is remarkably strong in scientific men—stronger than in others, I think. For this reason we must not always call a man dishonest because he does not yield to evidence for a long time. The old wall of belief must be broken down by much battering."

Kate Fox (now Mrs. Jencken) was free from domestic and maternal occupations, I feel compelled to suspend further investigation for the present.

To obtain free access to some persons abundantly endowed with the power I am experimenting upon, now involves more favour than a scientific investigator should be expected to make of it. Spiritualism amongst its more devout followers is a religion. The mediums, in many cases young members of the family, are guarded with a seclusion and jealousy which an outsider can penetrate with difficulty. Being earnest and conscientious believers in the truth of certain doctrines which they hold to be substantiated by what appear to them to be miraculous occurrences, they seem to hold the presence of scientific investigation as a profanation of the shrine. As a personal favour I have more than once been allowed to be present at meetings that presented rather the form of a religious ceremony than of a spiritualistic *séance*. But to be admitted by favour once or twice, as a stranger might be allowed to witness the Eleusinian mysteries, or a Gentile to peep within the Holy of Holies, is not the way to ascertain facts and discover laws. To gratify curiosity is one thing; to carry on systematic research is another. I am seeking the truth continually. On a few occasions, indeed, I have been allowed to apply tests and impose conditions; but only once or twice have I been permitted to carry off the priestess from her shrine, and in my own house, surrounded by my own friends, to enjoy opportunities of testing the phenomena I had witnessed elsewhere under less conclusive conditions.* My observations on these cases will find their due place in the work I am about to publish.

Following the plan adopted on previous occasions,—a plan which, however much it offended the prejudices of some critics, I have good reason to know was acceptable to the readers of the "Quarterly Journal of Science,"—I intended to embody the results of my labour in the form of one or two articles for this journal. However, on going over my notes, I find such a wealth of facts, such a superabundance of evidence, so overwhelming a mass of testimony, all of which will have to be marshalled in order, that I could fill several numbers of the "Quarterly." I must therefore be content on this occasion with an outline only of my labours, leaving proofs and full details to another occasion.

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* In this paper I give no instances and use no arguments drawn from these exceptional cases. Without this explanation it might be thought that the immense number of facts I have accumulated were principally obtained on the few occasions here referred to, and the objection would naturally arise of insufficiency of scrutiny from want of time.

My principal object will be to place on record a series of actual occurrences which have taken place in my own house, in the presence of trustworthy witnesses, and under as strict test conditions as I could devise. Every fact which I have observed is, moreover, corroborated by the records of independent observers at other times and places. It will be seen that the facts are of the most astounding character, and seem utterly irreconcilable with all known theories of modern science. Having satisfied myself of their *truth*, it would be moral cowardice to withhold my testimony because my previous publications were ridiculed by critics and others who knew nothing whatever of the subject, and who were too prejudiced to see and judge for themselves whether or not there was truth in the phenomena; I shall state simply what I have seen and proved by repeated experiment and test, and "I have yet to learn that it is irrational to endeavour to discover the causes of unexplained phenomena."

At the commencement, I must correct one or two errors which have taken firm possession of the public mind. One is that *darkness* is essential to the phenomena. This is by no means the case. Except where darkness has been a necessary condition, as with some of the phenomena of luminous appearances, and in a few other instances, everything recorded has taken place *in the light*. In the few cases where the phenomena noted have occurred in darkness I have been very particular to mention the fact; moreover some special reason can be shown for the exclusion of light, or the results have been produced under such perfect test conditions that the suppression of one of the senses has not really weakened the evidence.

Another common error is that the occurrences can be witnessed only at certain times and places,—in the rooms of the medium, or at hours previously arranged; and arguing from this erroneous supposition, an analogy has been insisted on between the phenomena called spiritual and the feats of legerdemain by professional "conjurors" and "wizards," exhibited on their own platform and surrounded by all the appliances of their art.

To show how far this is from the truth, I need only say that, with very few exceptions, the many hundreds of facts I am prepared to attest,—facts which to imitate by known mechanical or physical means would baffle the skill of a

Houdin, a Bosco, or an Anderson, backed with all the resources of elaborate machinery and the practice of years,—have all taken place in my own house, at times appointed by myself, and under circumstances which absolutely precluded the employment of the very simplest instrumental aids.

A third error is that the medium must select his own circle of friends and associates at a *séance;* that these friends must be thorough believers in the truth of whatever doctrine the medium enunciates; and that *conditions* are imposed on any person present of an investigating turn of mind, which entirely preclude accurate observation and facilitate trickery and deception. In reply to this, I can state that, (with the exception of the very few cases to which I have alluded in a previous paragraph* where, whatever might have been the motive for exclusiveness, it certainly was not the veiling of deception), I have chosen my own circle of friends, have introduced any hard-headed unbeliever whom I pleased, and have generally imposed my own terms, which have been carefully chosen to prevent the possibility of fraud. Having gradually ascertained some of the conditions which facilitate the occurrence of the phenomena, my modes of conducting these inquiries have generally been attended with equal, and, indeed, in most cases with more, success than on other occasions, where, through mistaken notions of the importance of certain trifling observances, the conditions imposed might render less easy the detection of fraud.

I have said that darkness is not essential. It is, however, a well-ascertained fact that when the force is weak a bright light exerts an interfering action on some of the phenomena. The power possessed by Mr. Home is sufficiently strong to withstand this antagonistic influence; consequently, he always objects to darkness at his *séances*. Indeed, except on two occasions, when, for some particular experiments of my own, light was excluded, everything which I have witnessed with him has taken place in the light. I have had many opportunities of testing the action of light of different sources and colours, such as sun-light, diffused day-light, moon-light, gas, lamp, and candle-light, electric light from a vacuum tube, homogeneous yellow light, &c. The interfering rays appear to be those at the extreme end of the spectrum.

I now proceed to classify some of the phenomena which have come under my notice, proceeding from the simple to the more complex, and briefly giving under each heading an outline of some of the evidence I am prepared to bring forward. My readers will remember that, with the exception of cases specially mentioned, the occurrences have taken place *in my own house, in the light, and with only private friends present* besides the medium. In the contemplated volume I propose to give in full detail the tests and precautions adopted on each occasion, with names of witnesses. I only briefly allude to them in this article.

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* See note on page 79.

CLASS I.

The Movement of Heavy Bodies with Contact, but without Mechanical Exertion.

This is one of the simplest forms of the phenomena observed. It varies in degree from a quivering or vibration of the room and its contents to the actual rising into the air of a heavy body when the hand is placed on it. The retort is obvious that if people are touching a thing when it moves, they push it, or pull it, or lift it; I have proved experimentally that this is not the case in numerous instances, but as a matter of evidence I attach little importance to this class of phenomena by itself, and only mention them as a preliminary to other movements of the same kind, but without contact.

These movements (and indeed I may say the same of every kind of phenomenon) are generally preceded by a peculiar cold air, sometimes amounting to a decided wind. I have had sheets of paper blown about by it, and a thermometer lowered several degrees. On some occasions, which I will subsequently give more in detail, I have not detected any actual movement of the air, but the cold has been so intense that I could only compare it to that felt when the hand has been within a few inches of frozen mercury.

CLASS II.

The Phenomena of Percussive and other Allied Sounds.

The popular name of "raps" conveys a very erroneous impression of this class of phenomena. At different times, during my experiments, I have heard delicate ticks, as with the point of a pin; a cascade of sharp sounds as from an induction coil in full work; detonations in the air; sharp metallic taps; a cracking like that heard when a frictional machine is at work; sounds like scratching; the twittering as of a bird, &c.

These sounds are noticed with almost every medium, each having a special peculiarity; they are more varied with Mr. Home, but for power and certainty I have met with no one who at all approached Miss Kate Fox. For several months I enjoyed almost unlimited opportunity of testing the various phenomena occurring in the presence of this lady, and I especially examined the phenomena of these sounds. With mediums, generally, it is necessary to sit for a formal *séance* before anything is heard; but in the case of Miss Fox it seems only necessary for her to place her hand on any substance for loud thuds to be heard in it, like a triple pulsation, sometimes loud enough to be heard several rooms off. In this manner I have heard them in a living tree—on a sheet of glass—on a stretched iron wire—on a stretched membrane—a tambourine—on the roof of a cab—and on the floor of a theatre. Moreover, actual contact is not always necessary; I have had these sounds proceeding from the floor, walls, &c., when the medium's hands and feet were held—when she was standing on a chair—when she was suspended in a swing from the ceiling—when she was enclosed in a wire cage—and when she had fallen fainting on a sofa. I have heard them on a glass harmonicon—I have felt them on my own shoulder and under my own hands. I have heard them on a sheet of paper, held between the fingers by a piece of thread passed through one corner. With a full knowledge of the numerous theories which have been started, chiefly in America, to explain these sounds, I have tested them in every way that I could devise, until there has been no escape from the conviction that they were true objective occurrences not produced by trickery or mechanical means.

An important question here forces itself upon the attention. *Are the movements and sounds governed by intelligence?* At a very early stage of the enquiry, it was seen that the power producing the phenomena was not merely a blind force, but was associated with or governed by intelligence: thus the sounds to which I have just alluded will be repeated a definite number of times, they will come loud or faint, and in different places at request; and by a pre-arranged code of signals, questions are answered, and messages given with more or less accuracy.

The intelligence governing the phenomena is sometimes manifestly below that of the medium. It is frequently in direct opposition to the wishes of the medium: when a determination has been expressed to do something which might not be considered quite right, I have known urgent messages given to induce a reconsideration. The intelligence is sometimes of such a character as to lead to the belief that it does not emanate from any person present.

Several instances can be given to prove each of these statements, but the subject will be more fully discussed subsequently, when treating of the source of the intelligence.

CLASS III.

The Alteration of Weight of Bodies.

I have repeated the experiments already described in this Journal, in different forms, and with several mediums. I need not further allude to them here.

CLASS IV.

Movements of Heavy Substances when at a Distance from the Medium.

The instances in which heavy bodies, such as tables, chairs, sofas, &c. have been moved, when the medium has not been touching them, are very numerous. I will briefly mention a few of the most striking. My own chair has been twisted partly round, whilst my feet were off the floor. A chair was seen by all present to move slowly up to the table from a far corner, when all were watching it; on another occasion an arm chair moved to where we were sitting, and then moved slowly back again (a distance of about three feet) at my request. On three successive evenings a small table moved slowly across the room, under conditions which I had specially pre-arranged, so as to answer any objection which might be raised to the evidence. I have had several repetitions of the experiment considered by the Committee of the Dialectical Society to be conclusive, viz., the movement of a heavy table in full light, the chairs turned with their backs to the table, about a foot off, and each person kneeling on his chair, with hands resting over the backs of the chair, but not touching the table. On one occasion this took place when I was moving about so as to see how everyone was placed.

CLASS V.

The Rising of Tables and Chairs off the Ground, without Contact with any Person.

A remark is generally made when occurrences of this kind are mentioned, Why is it only tables and chairs which do these things? Why is this property peculiar to furniture? I might reply that I only observe and record facts, and do not profess to enter into the Why and Wherefore; but indeed it will be obvious that if a heavy inanimate body in an ordinary dining-room has to rise off the floor, it cannot very well be anything else but a table or a chair. That this propensity is not specially attached to furniture, I have abundant evidence; but, like other experimental demonstrators, the intelligence or power, whatever it may be, which produces these phenomena can only work with the materials which are available.

On five separate occasions, a heavy dining-table rose between a few inches and 1½ feet off the floor, under special circumstances, which rendered trickery impossible. On another occasion, a heavy table rose from the floor in full light, while I was holding the medium's hands and feet. On another occasion the table rose from the floor, not only when no person was touching it, but under conditions which I had pre-arranged so as to assure unquestionable proof of the fact.

CLASS VI.

The Levitation of Human Beings.

This has occurred in my presence on four occasions in darkness. The test conditions under which they took place were quite satisfactory, so far as the judgment was concerned; but ocular demonstration of such a fact is so necessary to disturb our pre-formed opinions as to "the naturally possible and impossible," that I will here only mention cases in which the deductions of reason were confirmed by the sense of sight.

On one occasion I witnessed a chair, with a lady sitting on it, rise several inches from the ground. On another occasion, to avoid the suspicion of this being in some way performed by herself, the lady knelt on the chair in such manner that its four feet were visible to us. It then rose about three inches, remained suspended for about ten seconds, and then slowly descended. At another time two children, on separate occasions, rose from the floor with their chairs, in full daylight, under (to me) most satisfactory conditions; for I was kneeling and keeping close watch upon the feet of the chair, and observing that no one might touch them.

The most striking cases of levitation which I have witnessed have been with Mr. Home. On three separate occasions have I seen him raised completely from the floor of the room. Once sitting in an easy chair, once kneeling on his chair, and once standing up. On each occasion I had full opportunity of watching the occurrence as it was taking place.

There are at least a hundred recorded instances of Mr. Home's rising from the ground, in the presence of as many separate persons, and I have heard from the lips of the three witnesses to the most striking occurrence of this kind—the Earl of Dunraven, Lord Lindsay, and Captain C. Wynne—their own most minute accounts of what took place. To reject the recorded evidence on this subject is to reject all human testimony whatever; for no fact in sacred or profane history is supported by a stronger array of proofs.

The accumulated testimony establishing Mr. Home's levitations is overwhelming. It is greatly to be desired that some person, whose evidence would be accepted as conclusive by the scientific world—if indeed there lives a person whose testimony *in favour* of such phenomena would be taken—would seriously and patiently examine these alleged facts. Most of the eye-witnesses to these levitations are now living, and would, doubtless, be willing to give their evidence. But, in a few years, such *direct* evidence will be difficult, if not impossible, to be obtained.

CLASS VII.

Movement of Various Small Articles without Contact with any Person.

Under this heading I propose to describe some special phenomena which I have witnessed. I can do little more here than allude to some of the more striking facts, all of which, be it remembered, have occurred under circumstances that render trickery impossible. But it is idle to attribute these results to trickery, for I would again remind my readers that what I relate has not been accomplished at the house of a medium, but in my own house, where preparations have been quite impossible. A medium, walking into my dining-room, cannot, while seated in one part of the room with a number of persons keenly watching him, by trickery make an accordion play in *my own* hand when I hold it keys downwards, or cause the same accordion to float about the room playing all the time. He cannot introduce machinery which will wave window-curtains or pull up Venetian blinds 8 feet off, tie a knot in a handkerchief and place it in a far corner of the room, sound notes on a distant piano, cause a card-plate to float about the room, raise a water-bottle and tumbler from the table, make a coral necklace rise on end, cause a fan to move about and fan the company, or set in motion a pendulum when enclosed in a glass case firmly cemented to the wall.

CLASS VIII.

Luminous Appearances.

These, being rather faint, generally require the room to be darkened. I need scarcely remind my readers again that, under these circumstances, I have taken proper precautions to avoid being imposed upon by phosphorised oil or other means. Moreover, many of these lights are such as I have tried to imitate artificially, but cannot.

Under the strictest test conditions, I have seen a solid self-luminous body, the size and nearly the shape of a turkey's egg, float noiselessly about the room, at one time higher than any one present could reach standing on tiptoe, and then gently descend to the floor. It was visible for more than ten minutes, and before it faded away it struck the table three times with a sound like that of a hard, solid body. During this time the medium was lying back, apparently insensible, in an easy chair.

I have seen luminous points of light darting about and settling on the heads of different persons; I have had questions answered by the flashing of a bright light a desired number of times in front of my face. I have seen sparks of light rising from the table to the ceiling, and again falling upon the table, striking it with an audible sound. I have had an alphabetic communication given by luminous flashes occurring before me in the air, whilst my hand was moving about amongst them. I have seen a luminous cloud floating upwards to a picture. Under the strictest test conditions, I have more than once had a solid, self-luminous, crystalline body placed in my hand by a hand which did not belong to any person in the room. *In the light*, I have seen a luminous cloud hover over a heliotrope on a side table, break a sprig off, and carry the sprig to a lady; and on some occasions I have seen a similar luminous cloud visibly condense to the form of a hand and carry small objects about. These, however, more properly belong to the next class of phenomena.

CLASS IX.

The Appearance of Hands, either Self-Luminous or Visible by Ordinary Light.

The forms of hands are frequently *felt* at dark *séances*, or under circumstances where they cannot be seen. More rarely I have *seen* the hands. I will here give no instances in which the phenomenon has occurred in darkness, but will simply select a few of the numerous instances in which I have seen the hands in the light.

A beautifully-formed small hand rose up from an opening in a dining-table and gave me a flower; it appeared and then disappeared three times at intervals, affording me ample opportunity of satisfying myself that it was as real in appearance as my own. This occurred in the light in my own room, whilst I was holding the medium's hands and feet.

On another occasion, a small hand and arm, like a baby's, appeared playing about a lady who was sitting next to me. It then passed to me and patted my arm and pulled my coat several times.

At another time, a finger and thumb were seen to pick the petals from a flower in Mr. Home's button-hole, and lay them in front of several persons who were sitting near him.

A hand has repeatedly been seen by myself and others playing the keys of an accordion, both of the medium's hands being visible at the same time, and sometimes being held by those near him.

The hands and fingers do not always appear to me to be solid and life-like. Sometimes, indeed, they present more the appearance of a nebulous cloud partly condensed into the form of a hand. This is not equally visible to all present. For instance, a flower or other small object is seen to move; one person present will see a luminous cloud hovering over it, another will detect a nebulous-looking hand, whilst others will see nothing at all but the moving flower. I have more than once seen, first an object move, then a luminous cloud appear to form about it, and, lastly, the cloud condense into shape and become a perfectly-formed hand. At this stage, the hand is visible to all present. It is not always a mere form, but sometimes appears perfectly life-like and graceful, the fingers moving and the flesh apparently as human as that of any in the room. At the wrist, or arm, it becomes hazy, and fades off into a luminous cloud.

To the touch, the hand sometimes appears icy cold and dead, at other times, warm and life-like, grasping my own with the firm pressure of an old friend.

I have retained one of these hands in my own, firmly resolved not to let it escape. There was no struggle or effort made to get loose, but it gradually seemed to resolve itself into vapour, and faded in that manner from my grasp.

CLASS X.

Direct Writing.

This is the term employed to express writing which is not produced by any person present. I have had words and messages repeatedly written on privately-marked paper, under the most rigid test conditions, and have heard the pencil moving over the paper in the dark. The conditions—pre-arranged by myself—have been so strict as to be equally convincing to my mind as if I had seen the written characters formed. But as space will not allow me to enter into full particulars, I will merely select two instances in which my eyes as well as ears were witnesses to the operation.

The first instance which I shall give took place, it is true, at a dark *séance*, but the result was not less satisfactory on that account. I was sitting next to the medium, Miss Fox, the only other persons present being my wife and a lady relative, and I was holding the medium's two hands in one of mine, whilst her feet were resting on my feet. Paper was on the table before us, and my disengaged hand was holding a pencil.

A luminous hand came down from the upper part of the room, and after hovering near me for a few seconds, took the pencil from my hand, rapidly wrote on a sheet of paper, threw the pencil down, and then rose up over our heads, gradually fading into darkness.

My second instance may be considered the record of a failure. "A good failure often teaches more than the most successful experiment." It took place in the light, in my own room, with only a few private friends and Mr. Home present. Several circumstances, to which I need not further allude, had shown that the power that evening was strong. I therefore expressed a wish to witness the actual production of a written message such as I had heard described a short time before by a friend. Immediately an alphabetic communication was made as follows—"We will try." A pencil and some sheets of paper had been lying on the centre of the table; presently the pencil rose up on its point, and after advancing by hesitating jerks to the paper fell down. It then rose and again fell. A third time it tried, but with no better result. After three unsuccessful attempts, a small wooden lath, which was lying near upon the table, slid towards the pencil, and rose a few inches from the table; the pencil rose again, and propping itself against the lath, the two together made an effort to mark the paper. It fell, and then a joint effort was again made. After a third trial the lath gave it up and moved back to its place, the pencil lay as it fell across the paper, and an alphabetic message told us —"We have tried to do as you asked, but our power is exhausted."

CLASS XI.

Phantom Forms and Faces.

These are the rarest of the phenomena I have witnessed. The conditions requisite for their appearance appear to be so delicate, and such trifles interfere with their production, that only on very few occasions have I witnessed them under satisfactory test conditions. I will mention two of these cases.

In the dusk of the evening, during a *séance* with Mr. Home at my house, the curtains of a window about eight feet from Mr. Home were seen to move. A dark, shadowy, semi-transparent form, like that of a man, was then seen by all present standing near the window, waving the curtain with his hand. As we looked, the form faded away and the curtains ceased to move.

The following is a still more striking instance. As in the former case, Mr. Home was the medium. A phantom form came from a corner of the room, took an accordion in its hand, and then glided about the room playing the instrument. The form was visible to all present for many minutes, Mr. Home also being seen at the same time. Coming rather close to a lady who was sitting apart from the rest of the company, she gave a slight cry, upon which it vanished.

CLASS XII.

Special Instances which seem to point to the Agency of an Exterior Intelligence.

It has already been shown that the phenomena are governed by an intelligence. It becomes a question of importance as to the source of that intelligence. Is it the intelligence of the medium, of any of the other persons in the room, or is it an exterior intelligence? Without wishing at present to speak positively on this point, I may say that whilst I have observed many circumstances which appear to show that the will and intelligence of the medium have much to do with the phenomena,* I have observed some circumstances which seem conclusively to point to the agency of an outside intelligence, not belonging to any human being in the room. Space does not allow me to give here all the arguments which can be adduced to prove these points, but I will briefly mention one or two circumstances out of many.

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* I do not wish my meaning to be misunderstood. What I mean is, *not* that the medium's will and intelligence are actively employed in any conscious or dishonest way in the production of the phenomena, but that they sometimes appear to act in an unconscious manner.

I have been present when several phenomena were going on at the same time, some being unknown to the medium. I have been with Miss Fox when she has been writing a message automatically to one person present, whilst a message to another person on another subject was being given alphabetically by means of "raps," and the whole time she was conversing freely with a third person on a subject totally different from either. Perhaps a more striking instance is the following:—

During a *séance* with Mr. Home, a small lath, which I have before mentioned, moved across the table to me, in the light, and delivered a message to me by tapping my hand; I repeating the alphabet; and the lath tapping me at the right letters. The other end of the lath was resting on the table, some distance from Mr. Home's hands.

The taps were so sharp and clear, and the lath was evidently so well under control of the invisible power which was governing its movements, that I said, "Can the intelligence governing the motion of this lath change the character of the movements, and give me a telegraphic message through the Morse alphabet by taps on my hand?" (I have every reason to believe that the Morse code was quite unknown to any other person present, and it was only imperfectly known to me). Immediately I said this, the character of the taps changed, and the message was continued in the way I had requested. The letters were given too rapidly for me to do more than catch a word here and there, and consequently I lost the message; but I heard sufficient to convince me that there was a good Morse operator at the other end of the line, wherever that might be.

Another instance. A lady was writing automatically by means of the planchette. I was trying to devise a means of proving that what she wrote was not due to "unconscious cerebration." The planchette, as it always does, insisted that, although it was moved by the hand and arm of the lady, the *intelligence* was that of an invisible being who was playing on her brain as on a musical instrument, and thus moving her muscles. I therefore said to this intelligence, "Can you see the contents of this room?"

"Yes," wrote the planchette. "Can you see to read this newspaper?" said I, putting my finger on a copy of the *Times*, which was on a table behind me, but without looking at it. "Yes" was the reply of the planchette. "Well," I said, "if you can see that, write the word which is now covered by my finger, and I will believe you." The planchette commenced to move. Slowly and with great difficulty, the word "however" was written. I turned round and saw that the word "however" was covered by the tip of my finger.

I had purposely avoided looking at the newspaper when I tried this experiment, and it was impossible for the lady, had she tried, to have seen any of the printed words, for she was sitting at one table, and the paper was on another table behind, my body intervening.

CLASS XIII.

Miscellaneous Occurrences of a Complex Character.

Under this heading I propose to give several occurrences which cannot be otherwise classified owing to their complex character. Out of more than a dozen cases, I will select two. The first occurred in the presence of Miss Kate Fox. To render it intelligible, I must enter into some details.

Miss Fox had promised to give me a *séance* at my house one evening in the spring of last year. Whilst waiting for her, a lady relative, with my two eldest sons, aged fourteen and eleven, were sitting in the dining-room where the *séances* were always held, and I was sitting by myself, writing in the library. Hearing a cab drive up and the bell ring, I opened the door to Miss Fox, and took her directly into the dining-room. She said she would not go upstairs, as she could not stay very long, but laid her bonnet and shawl on a chair in the room. I then went to the dining-room door, and telling the two boys to go into the library and proceed with their lessons, I closed the door behind them, locked it, and (according to my usual custom at *séances*) put the key in my pocket.

We sat down, Miss Fox being on my right hand and the other lady on my left. An alphabetic message was soon given to turn the gas out, and we thereupon sat in total darkness, I holding Miss Fox's two hands in one of mine the whole time. Very soon, a message was given in the following words, "We are going to bring something to show our power;" and almost immediately afterwards, we all heard the tinkling of a bell, not stationary, but moving about in all parts of the room; at one time by the wall, at another in a further corner of the room, now touching me on the head, and now tapping against the floor. After ringing about the room in this manner for fully five minutes, it fell upon the table close to my hands.

During the time this was going on, no one moved and Miss Fox's hands were perfectly quiet. I remarked that it could not be my little hand-bell which was ringing, for I left that in the library. (Shortly before Miss Fox came, I had occasion to refer to a book, which was lying on a corner of a book-shelf. The bell was on the book, and I put it on one side to get the book. That little incident had impressed on my mind the fact of the bell being in the library.) The gas was burning brightly in the hall outside the dining-room door, so that this could not be opened without letting light into the room, even had there been an accomplice in the house with a duplicate key, which there certainly was not.

I struck a light. There, sure enough, was my own bell lying on the table before me. I went straight into the library. A glance showed that the bell was not where it ought to have been. I said to my eldest boy, "Do you know where my little bell is?" "Yes, papa," he replied, "there it is," pointing to where I had left it. He looked up as he said this, and then continued, "No—it's not there, but it was there a little time ago." "How do you mean?—has anyone come in and taken it?" "No," said he, "no one has been in; but I am sure it was there, because when you sent us in here out of the dining-room, J. (the youngest boy) began ringing it so that I could not go on with my lessons, and I told him to stop." J. corroborated this, and said that, after ringing it, he put the bell down where he had found it.

The second circumstance which I will relate occurred in the light, one Sunday evening, only Mr. Home and members of my family being present. My wife and I had been spending the day in the country, and had brought home a few flowers we had gathered. On reaching home, we gave them to a servant to put them in water. Mr. Home came soon after, and we at once proceeded to the dining-room. As we were sitting down, a servant brought in the flowers which she had arranged in a vase. I placed it in the centre of the dining-table, which was without a cloth. This was the first time Mr. Home had seen these flowers.

After several phenomena had occurred, the conversation turned upon some circumstances which seemed only explicable on the assumption that matter had actually passed through a solid substance. Thereupon a message was given by means of the alphabet: "It is impossible for matter to pass through matter, but we will show you what we can do." We waited in silence. Presently a luminous appearance was seen hovering over the bouquet of flowers, and then, in full view of all present, a piece of china-grass 15 inches long, which formed the centre ornament of the bouquet, slowly rose from the other flowers, and then descended to the table in front of the vase between it and Mr. Home. It did not stop on reaching the table, but went straight through it, and we all watched it till it had entirely passed through. Immediately on the disappearance of the grass, my wife, who was sitting near Mr. Home, saw a hand come up from under the table between them, holding the piece of grass. It tapped her on the shoulder two or three times with a sound audible to all, then laid the grass on the floor, and disappeared. Only two persons saw the hand, but all in the room saw the piece of grass moving about as I have described. During the time this was taking place, Mr. Home's hands were seen by all to be quietly resting on the table in front of him. The place where the grass disappeared was 18 inches from his hands. The table was a telescope dining-table, opening with a screw; there was no leaf in it, and the junction of the two sides formed a narrow crack down the middle. The grass had passed through this chink, which I measured, and found to be barely ⅛th inch wide. The stem of the piece of grass was far too thick to enable me to force it through this crack without injuring it, yet we had all seen it pass through quietly and smoothly; and on examination, it did not show the slightest signs of pressure or abrasion.

THEORIES TO ACCOUNT FOR THE PHENOMENA OBSERVED.

First Theory.—The phenomena are all the results of tricks, clever mechanical arrangements, or legerdemain; the mediums are impostors, and the rest of the company fools.

It is obvious that this theory can only account for a very small proportion of the facts observed. I am willing to admit that some so-called mediums of whom the public have heard much are arrant impostors who have taken advantage of the public demand for spiritualistic excitement to fill their purses with easily earned guineas; whilst others who have no pecuniary motive for imposture are tempted to cheat, it would seem, solely by a desire for notoriety. I have met with several cases of imposture, some very ingenious, others so palpable, that no person who has witnessed the genuine phenomena could be taken in by them. An enquirer into the subject finding one of these cases at his first initiation is disgusted with what he detects at once to be an imposture; and he not unnaturally gives vent to his feelings, privately or in print, by a sweeping denunciation of the whole genus "medium." Again, with a thoroughly genuine medium, the first phenomena which are observed are generally slight movements of the table, and faint taps under the medium's hands or feet. These of course are quite easy to be imitated by the medium, or anyone at the table. If, as sometimes occurs, nothing else takes place, the sceptical observer goes away with the firm impression that his superior acuteness detected cheating on the part of the medium, who was consequently afraid to proceed with any more tricks in *his* presence. He, too, writes to the newspapers exposing the whole imposture, and probably indulges in moral sentiments about the sad spectacle of persons, apparently intelligent, being taken in by imposture which he detected at once.

There is a wide difference between the tricks of a professional conjurer, surrounded by his apparatus, and aided by any number of concealed assistants and confederates, deceiving the senses by clever sleight of hand on his own platform, and the phenomena occurring in the presence of Mr. Home, which take place in the light, in a private room that almost up to the commencement of the *séance* has been occupied as a living room, and surrounded by private friends of my own, who not only will not countenance the slightest deception, but who are watching narrowly every thing that takes place. Moreover, Mr. Home has frequently been searched before and after the *séances*, and he *always* offers to allow it. During the most remarkable occurrences I have occasionally held both his hands, and placed my feet on his feet. On no single occasion have I proposed a modification of arrangements for the purpose of rendering trickery less possible which he has not at once assented to, and frequently he has himself drawn attention to tests which might be tried.

I speak chiefly of Mr. Home, as he is so much more powerful than most of the other mediums I have experimented with. But with all I have taken such precautions as place trickery out of the list of possible explanations.

Be it remembered that an explanation to be of any value must satisfy *all* the conditions of the problem. It is not enough for a person, who has perhaps seen only a few of the inferior phenomena, to say "I suspect it was all cheating," or, "I saw how some of the tricks could be done."

Second Theory.—The persons at a *séance* are the victims of a sort of mania or delusion, and imagine phenomena to occur which have no real objective existence.

Third Theory.—The whole is the result of conscious or unconscious cerebral action.

These two theories are evidently incapable of embracing more than a small portion of the phenomena, and they are improbable explanations for even those. They may be dismissed very briefly.

I now approach the "Spiritual" theories. It must be remembered that the word "spirits" is used in a very vague sense by the generality of people.

Fourth Theory.—The result of the spirit of the medium, perhaps in association with the spirits of some or all of the people present.

Fifth Theory.—The actions of evil spirits or devils, personifying who or what they please, in order to undermine Christianity and ruin men's souls.

Sixth Theory.—The actions of a separate order of beings, living on this earth, but invisible and immaterial to us. Able, however, occasionally to manifest their presence. Known in almost all countries and ages as demons (not necessarily bad), gnomes, fairies, kobolds, elves, goblins, Puck, &c.

Seventh Theory.—The actions of departed human beings—the spiritual theory *par excellence.*

Eighth Theory.—(*The Psychic Force Theory*).—This is a necessary adjunct to the 4th, 5th, 6th, and 7th theories, rather than a theory by itself.

According to this theory the "medium," or the circle of people associated together as a whole, is supposed to possess a force, power, influence, virtue, or gift, by means of which intelligent beings are enabled to produce the phenomena observed. What these intelligent beings are is a subject for other theories.

It is obvious that a "medium" possesses a *something* which is not possessed by an ordinary being. Give this *something* a name. Call it "*x*" if you like. Mr. Serjeant Cox calls it Psychic Force. There has been so much misunderstanding on this subject that I think it best to give the following explanation in Mr. Serjeant Cox's own words:—

"The Theory of *Psychic Force* is in itself merely the recognition of the now almost undisputed fact that under certain conditions, as yet but imperfectly ascertained, and within a limited, but as yet undefined, distance from the bodies of certain persons having a special nerve organisation, a Force operates by which, without muscular contact or connection, action at a distance is caused, and visible motions and audible sounds are produced in solid substances. As the presence of such an organisation is necessary to the phenomenon, it is reasonably concluded that the Force does, in some manner as yet unknown, proceed from that organisation. As the organism is itself moved and directed within its structure by a Force which either is, or is controlled by, the Soul, Spirit, or Mind (call it what we may) which constitutes the individual being we term 'the Man,' it is anequally reasonable conclusion that the Force which causes the motions beyond the limits of the body is the same Force that produces motion within the limits of the body. And, inasmuch as the external force is seen to be often directed by Intelligence, it is an equally reasonable conclusion that the directing Intelligence of the external force is the same Intelligence that directs the Force internally. This is the force to which the name of *Psychic Force* has been given by me as properly designating a force which I thus contend to be traced back to the Soul or Mind of the Man as its source. But I, and all who adopt this theory of Psychic Force as being the agent through which the phenomena are produced, do not thereby intend to assert that this Psychic Force may not be sometimes seized and directed by some other Intelligence than the Mind of the Psychic. The most ardent Spiritualists practically admit the existence of Psychic Force under the very inappropriate name of Magnetism (to which it has no affinity whatever), for they assert that the Spirits of the Dead can only do the acts attributed to them by using the Magnetism (that is, the Psychic Force) of the Medium. The difference between the advocates of Psychic Force and the Spiritualists consists in this—that we contend that there is as yet insufficient proof of any other directing agent than the Intelligence of the Medium, and no proof whatever of the agency of Spirits of the Dead; while the Spiritualists hold it as a faith, not demanding further proof, that Spirits of the Dead are the sole agents in the production of all the phenomena. Thus the controversy resolves itself into a pure question of *fact*, only to be determined by a laborious and long-continued series of experiments and an extensive collection of psychological *facts*, which should be the first duty of the Psychological Society, the formation of which is now in progress."

NOTICES OF BOOKS.

Lectures on Light. Delivered in America in 1872-1873. By JOHN TYNDALL, LL.D., F.R.S., Professor of Natural Philosophy in the Royal Institution. London: Longmans, Green, and Co. 1873. 8vo., 268 pp.

WE are all more or less familiar with the history of Dr. Tyndall's recent visit to America. We know that he was invited to that country to give a course of lectures on Natural Philosophy in the principal cities of the States, and that he was received everywhere with open arms. The thirst of the Americans for science is prodigious; a considerable scientific taste and literature is springing up amongst them; their enterprise induces them to constantly reprint our large works on science; and it will be remembered that these very lectures of Dr. Tyndall's were printed in broadsides, illustrated, and issued, in a newspaper-like form, at the cost of a few cents. The subject chosen was Light, and, in the work before us, we have the course of six lectures on that subject, which Dr. Tyndall delivered in the principle centres of American thought and progress.

Dr. Tyndall has adopted the plan of giving a history of the Science of Light, and illustrating each fact as it was discovered. Thus early in the first lecture we find an account of the law, that the angle of incidence is equal to the angle of reflection; of Snell's Law of the Refraction of Light (1621), sometimes called 'Descartes' Law;' and of Rœmer's Determination of the Velocity of Light (1676). "Snell's Law of Refracton," says our author, "is one of the corner stones of optical science, and its applications to-day are millionfold. Immediately after its discovery, Descartes applied it to the explanation of the rainbow. A beam of solar light falling obliquely upon a raindrop is refracted on entering the drop, and, on emerging, is again refracted." Here follows an explanation of the rainbow, and the means by which Descartes proved its origin. Next we have an account of Newton's Discovery of the Decomposition and Recomposition of Light, and many illustrations of the discovery; then of Achromatism, and the Theory of Colours.

In the second lecture we are introduced to the once rival conjectures concerning the nature of light:—The "Emission Theory," and the "Undulatory Theory;" and here Dr. Tyndall introduces some very pertinent remarks regarding the conception of a physical theory, and the necessary use of the imagination in that form of conception:—"This conception of physical theory implies, as you perceive, the exercise of the imagination. Do not be afraid of this word, which seems to render so many respectable people, both in the ranks of science and out of them, uncomfortable. That men in the ranks of science should feel thus is, I think, a proof that they have suffered themselves to be misled by the popular definition of a great faculty, instead of observing its operation in their own minds. Without imagination we cannot take a step beyond the bourne of the mere animal world, perhaps not even to the edge of this. But, in speaking thus of imagination, I do not mean a riotous power which deals capriciously with facts, but a well ordered and disciplined power, whose sole function is to form conceptions which the intellect imperatively demands. Imagination thus exercised never really severs itself from the world of fact: This is the storehouse from which all its pictures are drawn; and the magic of its art consists, not in creating things anew, but in so changing the magnitude, position, and other relations of sensible things, as to render them fit for the requirements of the intellect in the subsensible world." The growth of the rival theories is then traced, and the use of imagination by those who favoured one or the other; we were unaware that Sir David Brewster did not adopt the undulatory theory, and certainly unaware that he permitted sentiment to interfere with his acceptation of a physical theory, as the following passage indicates:—"In one of my latest conversations with Sir David Brewster, he said to me that his chief objection to the undulatory theory of light was, that he could not think the Creator guilty of so clumsy a contrivance as the filling of space with ether in order to produce light. This, I may say, is very dangerous ground, and the quarrel of science with Sir David on this point, as with many estimable persons on other points, is, that they profess to know too much about the mind of the Creator." The elaborate and most due praise which Tyndall, and Helmholtz, and many others have bestowed upon Thomas Young here reappears. But when Dr. Tyndall tells us that Young was but a little lower in the intellectual scale than Newton, and greater than any man of science between Newton's time and his own, we cannot, with all possible love for our country and countrymen, hold with him. For he practically makes Newton the greatest man of science of all time, and Young second only to the greatest. Elsewhere he says "we trace the progress of astronomy through Hipparchus and Ptolemy; and after a long halt, through Copernicus, Galileo, Tycho Brahe, and Kepler; while from the high table-land of thought raised by these men Newton shoots upward like a peak, overlooking all others from his dominant elevation." And we would invite comparison between the great and glorious Leonardo da Vinci and the great and glorious Dr. Thomas Young—men who had many remarkable points of contact. If no more, at least they should be "bracketed together" in the list of illustrious men, but we incline to the greater exaltation of Leonardo da Vinci.

The third lecture treats of the double refracton and polarisation of light. We may particularly call attention to Fig. 27, and the admirably clear explanation which is given of the cause of refraction by a prism, and of the change of wave-front. The polarised beam of light is compared, in the matter of its two-sidedness with the two-endedness of a magnet. The subject of polarisation, and the chromatic phenomena which accompany it, is continued in the fourth lecture; and the many admirable experimental illustrations with which Dr. Tyndall has made us familiar at the Royal Institution are freely introduced to develope the subject more clearly.

The fifth lecture takes us into Dr. Tyndall's more special domain of radiant heat, and the consideration of its relationship to light. The subject of Fluorescence is fully discussed, and the usual solution of sulphate of quinine, and the recently discovered *Thallene* of Prof. Morton used to illustrate it. Then we have an interesting paragraph relating to what is now called "Potential Light." "Fluor-spar, and some other substances, when raised to a temperature still under redness, emit light. During the ages which have elapsed since their formation, this capacity of shaking the ether into visual tremors appears to have been enjoyed by these substances. Light has been potential within them all this time; and, as well explained by Draper, the heat, though not itself of visual intensity, can unlock the molecules so as to enable them to exert the power of vibration which they possess." The observations of Dr. Bence Jones appear to have proved the existence of a fluorescent substance in the human body, notably in the lens of the eye. Thus, if the eye be plunged into the ultra-violet rays, it becomes conscious of a "whitish-blue shimmer" filling the space before it; and, according to Dr. Tyndall, the crystalline lens of the eye, if it be then viewed from without, is seen to gleam vividly. An account of *calorescence*, and of the effects of the non-luminous ultra-red rays is next given, and, near the end of the lecture, of the refraction, polarisation, and magnetisation of heat. We cannot completely understand the connection between the ultra-red rays and certain phenomena in Nature (described on pp. 176 and 177, and could almost imagine that a page or paragraph had been omitted. We can find nothing in the text to prove that the invisible ultra-red rays produce the warming and consequent evaporation of the tropical oceans, and hence, indirectly, our rains and snows. Again, a large flask containing a freezing mixture, and coated with hoar-frost, was placed at an intensely luminous focus of electric light, an alum-cell being interposed; the frost was not melted: when, however, an iodine-cell, which cut off all the light, was interposed, and the alum-cell withdrawn, the hoar-frost was melted. "Hence," says our author, "we infer that the snow and ice which feed the Rhone, the Rhine, and other rivers which have glaciers for their sources, are released from their imprisonment upon the mountains by the invisible ultra-red rays of the sun." Why *hence?* The experiment indeed proves, that if you cut off all the heat rays, the luminous rays possess no warmth; while, if you cut off all the luminous rays, the heat rays exercise great power; but it does not prove to us that luminous heat plays no part in the phenomena of Nature. A word or two of amplification, which considerably simplify this part of the lecture.

The sixth and final lecture is devoted to the important and rapidly growing subject of Spectrum Analysis, to which we need not very specially refer. This lecture is terminated by no less than nineteen pages of summary and conclusion. From this we may with advantage note here and there, of necessity somewhat desultorily, a remark or a generalisation. Dr. Tyndall speaks of Newton's emission theory in the following terms: "For a century it stood like a dam across the course of discovery; but, like all barriers that rest upon authority and not upon truth, the pressure from behind increased, and swept the barrier away." Of the undulatory theory, he says, "It had been enunciated by Hooke, it had been applied by Huyghens, it had been defended by Euler. But they made no impression. . . . . It first took the form of a demonstrated verity in the hands of Thomas Young. . . . After him came Fresnel, whose transcendent mathematical abilities enabled him to give the theory a generality unattained by Young. He grasped the theory in its entirety; followed the ether into the hearts of crystals of the most complicated structure, and into bodies subjected to strain and pressure. He showed that the facts discovered by Malus, Arago, Brewster, and Biot, were so many ganglia, so to speak, of his theoretic organism, deriving from it sustenance and explanation."

In his concluding remarks, Dr. Tyndall has addressed to the Americans some admirable remarks concerning the so-called practical scientific man and the original investigator. He has begged them not to confound the two, not, as is too often the case, to attribute the discoveries of the original worker to the man who applies them to the practical good of mankind. And he has besought them to endeavour to foster and cultivate original research. "Your most difficult problem will be not to build institutions, but to discover men. You may erect laboratories and endow them, you may furnish them with all the appliances needed for enquiry; in so doing you are but creating opportunity for the exercise of powers which come from sources entirely beyond your reach. You cannot create genius by bidding for it. . . . . . You have scientific genius amongst you—not sown broadcast, believe me, it is sown thus nowhere—but still scattered here and there. Take all unnecessary impediments out of its way. Keep your sympathetic eye upon the originator of knowledge. Give him the freedom necessary for his researches, not overloading him either with the duties of tuition or of administration, not demanding from him so-called practical results—above all things avoiding that question which ignorance so often addresses to genius, 'What is the use of your work?' Let him make truth his object, however unpractical for the time being, that truth may appear. If you cast your bread thus upon the waters, then be assured it will return to you, though it may be after many days."

We trust our American cousins, or, as we should prefer to call them "brothers, speaking the same dear mother tongue," will lay all this to heart, and let it bear good fruit. We rejoice to know that one of our own scientific men has been received by the Americans, as Dr. Tyndall has been received, and we trust that the establishment of these social relationships will do much to bind together the two great countries into still closer union.

The Spectroscope and its Applications. By J. NORMAN LOCKYER, F.R.S. Macmillan and Co. London: 1873. 8vo., 117 pp. Illustrated.

THIS is the first volume of a series of popular works on Science, to be called the *Nature Series*, because the subject-matter is first printed in "Nature." Eight of these books are already announced, and some two or three will no doubt be ready by next October. The design is good, and the books will probably supply a want which is being felt in this country and America. Judging from the present volume, the series will resemble Messrs. Hachette's *Librarie des Merveilles* more closely than any other works in our language. The general appearance, as to externals, is altogether prepossessing—the book is well printed on thick paper, profusely illustrated, and very neatly bound.

The present volume consists of three lectures on the Spectroscope, delivered before the Society of Arts in 1869. They here appear in a revived and somewhat expanded form, and the subject has, as far as possible, been brought up to the day of issue.

The first lecture regards the matter from an historical and descriptive point of view. The broad points of interest connected with the history of the spectroscope are clearly discussed. The proof that lights which differ in colour differ in refrangibility; that the light of the sun consists of rays possesssing different refrangibilities; the decomposition and recomposition of light. Newton, in his experiments, had used a round hole in a shutter for the admission of a beam of light; while Wollaston, in 1802, made what our author calls "a tremendous step in advance," by substituting a slit instead of a circular hole. This simple modification of Newton's experiment permitted a spectrum of considerable purity to be obtained for the colours, instead of overlapping, were now seen more distinctly side by side, and with only their edges overlapping. In this spectrum Wollaston found breaks of continuity, not observed by Newton; he discovered the black lines at right angles to the length of the spectrum, which we now call "Fraünhofer's lines." Ten years later, in 1812, the German optician, Fraünhofer mapped no less than 576 of these lines, and lettered the principal ones A, B, C, D; he discovered, moreover, that in the spectrum of certain stars, the black lines do not hold by any means the same position which they hold in the spectrum of the sun. In 1830, the next great improvement in the spectroscope was made. In addition to the simple prism and slit, Mr. Simms, the optician, placed a lens in front of the prism and another lens near the eye, so as to magnify the spectrum. By this means the black lines could be studied with far greater readiness than by the naked eye. "You may imagine the enormous mystery—the wonderful reverence almost—with which this question of the Fraünhofer's lines was approached until they were thoroughly understood; and recollect that we owe the discovery of them, by which we are enabled now to determine the pressures acting in the atmospheres of the most distant stars, simply to the fact that Dr. Wollaston, instead of drilling a round hole, used a slit; and to the other additional fact, that Mr. Simms, instead of using that slit with a mere prism, used a lens, and made the beam parallel, and then allowed that parallel beam, after it had passed through the prism, to pass into another telescope, and form an image of the slit for each ray. You see how closely connected are the grandest discoveries with the skill and suggestiveness of those who supply different instruments for our use." Thus the principal incidents in the history of the spectroscope, as an instrument, are (*a*) Newton's application of the prism to optical purposes, in 1675; (*β*) his observation that the prism should be used at the angle of minimum deviation; (*γ*) the addition of the slit by Wollaston in 1812; (*δ*) the collimating lens added by Simms in 1830. The author next describes various forms of spectroscopes with one, two, or more prisms. Capital figures are given of Steinheil's four-prism spectroscope, used by Kirchhoff; of Huggins's star spectroscope, and of direct vision spectroscopes with three or four prisms.

The second lecture treats of the applications of the spectroscope, specially of those which depend upon the investigation of light radiated from bodies. Thus applied, the instrument enables us to distinguish between solids, liquids, and gases; and between gases and vapours existing at different pressures. The various methods of obtaining spectra are here discussed; the use of the Bunsen burner, the induction coil, and the voltaic arc. A coloured plate at the commencement of the volume shows, among other things, two spectra of great interest; the one of hydrogen at a high pressure, the other of hydrogen at a low pressure. It is here seen that, in the former instance, the spectrum is much more continuous than in the latter. In fact, in the vacuous tube, the hydrogen spectrum dwindles down to one or two very sharply defined and thin lines. On the other hand, Frankland has proved that the spectrum of incandescent hydrogen existing under very great pressure is entirely continuous. For intermediate pressures we have intermediate spectra. "We may state generally," says the author, "that beginning with any one element in its most rarefied condition, and then following its spectrum, as the molecules come nearer together, so as at last to reach the solid form, we shall find that spectrum become more and more complicated as this approach takes place, until at last a vivid continuous spectrum is reached." The latter part of this lecture treats of the application of the spectroscope to the investigation of the nature of various heavenly bodies, the sun, stars, and nebulæ, at the hands of various physicists and astronomers. The third and concluding lecture mainly discusses absorption-spectra. It is here shown that it easy to detect different substances by noticing their absorption; by introducing the substance between the source of light and the prism, and noticing the resulting spectrum, side by side with a continuous spectrum. We may discriminate between two solutions, the one of blood, the other of magenta dye, exactly similar in colour and general appearance. Again, Mr. Sorby has proved that, by means of the spectrum-microscope, a blood spot so small that it contains only one-thousandth of a grain of blood may be detected. Some very interesting and comparatively new matter is given near the end of this lecture relating to the probably constitution of sun spots, the spectroscopic examination of which seems to prove that they are due to "*general* absorption, plus *special* absorption in some particular lines." The observed widening of the sodium line in the spectrum of a sun spot is traced to difference of pressure:—"If we take a tube containing some metallic sodium sealed up in hydrogen, and pass a beam of light from the electric lamp through it, by decomposing this beam with our prisms we shall obtain an ordinary continuous spectrum without either bright or dark lines; but by heating the metallic sodium in the tube, which is placed in front of the slit, we really fill that tube with the vapour of sodium; and as the heating will be slow, the sodium vapour will rise very gently from the metal at the bottom, so that we shall get layers of different densities of sodium vapour filling the tube. Immediately the sodium begins to rise in vapour, a black absorption-line shows itself, in one spectrum, in precisely the same position as the yellow line of sodium, and you will find that the thickness of the sodium absorption line will vary with the density of the stratum of vapour through which it passes. Thus, from the upper part of the tube we obtain a fine delicate line, which gradually thickens as we approach the bottom of the tube, and thus we produce the appearance in the spectrum of the spot where the layers of sodium vapour are very dense, and the very fine delicate line of the sodium vapour when thrown up into the sun's chromosphere." Thus, in fact, it seems to be undoubted that, just as the hydrogen line widens as the surface of the sun is approached, indicating increase of pressure, so, in the sun-spot, the pressure of the incandescent sodium vapour increases as we go deeper into the sun-spot cavity. Finally we have an account of the velocity of solar storms, calculated from the shifting of the F line, to be something like a hundred miles in a second.

Altogether the work is very readable, and it will form a useful introduction to larger works, such as that of Roscoe or Schellen. It shows, however, frequent evidence of haste, both in style of composition and in arrangement of subject-matter, and several somewhat abstruse subjects receive rather slight treatment. We regret also to notice the comparatively slight mention of foreign investigators, and of our own Miller, Brewster, and Herschel. But we must bear in mind that the subject is vast, and three hours of exposition are very insufficient for a detailed account of only one branch of it. We commend the work to all who are interested in one of the most prominent branches of optical science of our day, and remind them that it is the work of one who has done good service to science by his own researches.

Light Science for Leisure Hours. Second Series. Familiar Essays on Scientific Subjects, Natural Phenomena, &c., with a Sketch of the Life of Mary Somerville. By RICHARD A. PROCTOR, B.A., Cambridge, Honorary Secretary of the Royal Astronomical Society, &c. London: Longmans, Green, and Co. 1873.

SOME years ago a caricature portrait of Alexandre Dumas was published in Paris, in which the brilliant novelist was pictured with a bunch of hands attached to each arm, and each hand carrying a pen that was scribbling furiously. The flood of books, magazine articles, controversial and other correspondence, besides communications and hard secretary's work to the Royal Astronomical Society, which, during the last few years, have poured from the pen of Mr. Proctor, render a similar caricature almost justifiable, but it demands some modification. Instead of merely a bunch of supplementary hands, the artist would, in this case, require to depict an efflorescence of supplementary brains, Mr. Proctor's work having been something considerably beyond the efforts of a mere book-maker. He has specially attacked the grandest and heaviest of the sciences, and made it the leading subject of his popular teaching; but, instead of re-baking the old dishes of his predecessors, he has laid before his readers the most recently discovered facts, generalisations, and speculations of modern astronomy; a large proportion of which, according to the old method of dishing up astronomical handbooks, would have remained during another generation or so buried in the Transactions of learned societies, or in the unread volumes of those who might be so simple-minded as to publish, on their own account, important contributions to astronomical science, under the delusion that they would be studied during their own lifetime.

Mr. Proctor has the high merit—very rare among avowedly popular teachers—of digging, with his own hands, into these depths of astronomical literature, and of directly presenting to readers of all classes judiciously selected and well displayed examples of their treasures.

The second series of "Light Science for Leisure Hours" is one of the latest of these collections of nuggets (that is up to this moment of writing; we cannot tell what may happen during the short time that will elapse before this is published), and is fully equal in interest and value to its predecessors.

A considerable portion of the volume is devoted to meteorological problems, and the interminable "Gulf Stream" discussion, into the lists of which tournament Mr. Proctor has valiantly entered, the device on his shield being surface evaporation over large tropical areas. He repeats Maury's demonstration of the insufficiency of Herschel's trade wind explanation, and Herschel's refutation of Maury's and Humboldt's variation of the specific gravity theory, and then proceeds to show that Dr. Carpenter's lump of ice, at one end of a rectangular aquarium trough, is a fallacious representation of the arctic ice in arctic waters, inasmuch as the arctic area is so much smaller than the tropical that the trough should have been V-shaped, with the ice at the angle, in order to be at all representative. There can be no doubt that this simple quantitative difficulty is fatal to Dr. Carpenter's large estimate of the potency of the arctic ice-cold stream.

Taking a cool outside view of this controversy, it presents one very interesting feature, namely that each combatant succeeds in refuting the *sufficiency* of his opponents explanation, but (as far as those above-named are concerned) all have failed to refute its *actuality*. Hence we may venture to conclude that the errors on all sides are quantitative only, and that the actual oceanic circulation is due to the combined, or rather co-operating action, of all the forces on behalf of which the champions are respectively combating. We say no more, lest the infection of the fight should come upon us and deform our critical impartiality.

The other essays are on "The Coming Transit of Venus" of course, "The Ever Widening World of Stars," Movements in the Star Depths," "The Great Nebula in Orion," "The Sun's True Atmosphere," "Something Wrong with the Sun," "News from Herschel's Planet," "The Two Comets of 1868," "Comets of Short Period," "The Climate of Great Britain," "The Low Barometer of the Antarctic Temperate Zone."

We cannot of course describe or discuss the contents of such a series. They are all written with Mr. Proctor's usual graphic clearness, and carry the reader forward with the latest steps in the progress of those departments of science which they treat.

Besides these, there is a critical sketch of the life and works of Mrs. Somerville.

In his preface Mr. Proctor invites special attention to the essay on the Transit of Venus, and expresses his practical conclusion on this subject in unmistakable and uncompromising terms, in italics, thus, that "there is great risk that, for want of an adequate number of southern stations, *the whole series of observations, by all countries engaged in the work, will result in failure*," and he adds in a note that, "since this was written, I have received letters from the greatest master of mathematical astronomy this country has produced since Newton's day, strongly confirming my views as to the extreme importance of providing many southern stations for applying Halley's method in 1874, and urging me, moreover, to appeal to America to take part in this special work, for which she is peculiarly fitted, because of the bravery and enterprise of her seamen, the skill and ingenuity of her astronomers and physicists, and her singular liberality as a nation in all scientific matters."

Electricity and Magnetism. By FLEEMING JENKIN, F.R.SS. L. & E., M.I.C.E., Professor of Engineering in the University of Edinburgh. (Text-Books of Science.) London: Longmans, Green, and Co. 1873.

OF the great and increasing value of the series of text-books published by Messrs. Longmans, there cannot be the remotest doubt. Not only are the contents of each little work valuable to the student, but it becomes a pleasure to the proficient to see the principles of his science advanced so clearly and cleverly by the best expounders. Professor Fleeming Jenkin has done ably by electrical science in the above volume of the series; and particularly does he make clear the difficult subject of contact electricity. We commend the work to the notice of our readers interested in electrical science.

Quantitative Chemical Analysis. By T. E. THORPE, Ph., D., F.R.S.E. Professor of Chemistry, Andersonian Institution, Glasgow. London: Longmans, Green, and Co.

THIS work forms another of the "series of text-books of science, adapted for the use of artisans and students in public and other schools." Its speciality is that the "examples chosen" have been selected to a great extent "on account of their practical utility." In other words, the work has a technological character, which, without unfitting it for students of other classes, makes it particularly suited for artisans. It is divided into five sections. The first of these is a full and carefully compiled treatise on chemical manipulation, as far as qualitative analysis is concerned. The instructions on the use of the balance are admirable, and it would not be easy to point out any necessary precaution which the author has omitted to press on the attention of his readers. Indeed it might be suggested that mention of the method of weighing by vibrations, with the accompanying mathematical formulæ, are not somewhat out of place in such a work. There is also an attempt at determining by algebraical calculations the amount of wash-water, and the minimum number of washings required to bring any precipitate to a state of purity. The use of the filter-pump is explained at full length. The action of the solvents employed upon the apparatus in digestions, evaporations, &c., is pointed out as a possible, though generally overlooked, source of error. In short, not merely students—in the common sense of the term—but not a few chemists of old standing might find their advantage in a careful perusal of this section.

The remainder of the work consists of a graduated series of examples in simple gravimetric analysis; of a section on volumetric operations; of an account of the methods used in the valuation of ores, minerals, and industrial products; and lastly, of a special section on organic analysis.

Turning to the assay of pyrites, we find that the author directs the sulphur to be oxidised by means of chlorate of potassa, and not hydrochloric acid—as a certain standard work advises, with the ordinary result of a small explosion—but nitric acid, which works safely and well. The subsequent expulsion of the nitric acid, the rendering all silica insoluble, the removal of the last traces of sulphate of lead which may possibly be present, are all duly pointed out as needful. The addition of a little tartaric acid to prevent the precipitation of iron, along with the sulphate of baryta, is a sound precaution. We have seen precipitates of sulphate of baryta which, though thrown down from decidedly acid solutions, had a very tawny look. The digestion with acetate of ammonia to remove traces of nitrate of baryta which may remain entangled in the precipitate is also useful.

Turning to the separation of phosphoric acid from iron and alumina, we find that the author recommends the tin process (Reynoso's) to the exclusion both of the bismuth and of the molybdenum method, the latter of which we certainly prefer, whenever a comparatively small amount of phosphoric acid has to be determined in prescence of a large proportion of iron and alumina. The molybdenum process is, however, described and recommended for the determination of phosphorus in irons and iron ores. It is remarkable how generally chemists overlook the fact that, when a substance has been ignited to remove organic matter, its phosphoric acid will be to a great extent converted into pyrophosphoric acid. Unless special precautions are taken for its re-conversion, the amount of phosphoric acid found will be decidedly erroneous.

The analysis of copper ores is very fully and clearly explained. Now methods so accurate, convenient, and rapid as the "Mansfield" and "Luckow's" are known, the question arises why the Cornish dry assay, which gives tolerably correct results only in the case of rich ores, should still be recognised?

Space will not permit us to examine this work at greater length. But although on certain points we should differ to some extent from the author, we can conscientiously recommend the work, not merely as a text-book for the student, but as a useful manual of reference to advanced and experienced chemists.

Workshop Appliances; including Descriptions of the Gauging and Measuring Instruments, the Hand-Cutting Tools, Lathes, Drilling, Planing, and other Machine Tools used by Engineers. By C. P. B. Shelley, C.E., Honorary Fellow of and Professor of Manufacturing Art and Machinery in King's College, London. London: Longmans, Green, and Co. 1873.

This also forms one of the series of Text-Books of Science, in course of publication by Messrs. Longmans, Green, and Co.; the subject embraced is, however, far too vast to be properly comprised within the pages of a single volume. As a text-book for novices, it may be of some value; and that part which relates to the use of hand tools will be found instructive, but it is the only portion of it which possesses any originality. The first chapter, "On Measures of Length, and Methods of Measuring," is almost a reprint from another source, but it is interesting, and forms a good introduction to the subjects subsequently treated of. The description of "wire gauges" is defective, inasmuch as no mention is made of Mr. Latimer Clark's proposed gauge, with which the author should be well acquainted. The latter half of the book, which relates to machine tools, is of but little use; the descriptive part is deficient, the mechanical details are insufficiently explained, and many of the illustrations are from old worn-out blocks which we recognise as having repeatedly seen in manufacturers' illustrated trade circulars. This portion might well have formed the subject-matter for a separate volume, and no doubt, if more space had been available for the purpose, Mr. Shelley would have done it fuller justice than he has been able to do within the limits to which he appears to have been confined.

Sanitary Engineering: a Guide to the Construction of Works of Sewerage and House Drainage; with Tables for Facilitating the Calculations of the Engineer. By BALDWIN LATHAM, C.E., M. Inst. C.E. London: E. and F. N. Spon. 1873.

MR. BALDWIN LATHAM is a well-known authority on all matters relating to Sanitary Engineering, and his experience in the construction of town sewers and the disposal of sewage has been such as to enable him to write from actual experience upon the subjects treated of in the volume now before us. Much of the information given in this book has appeared piecemeal elsewhere, having formed the subject of papers read before the Society of Engineers, and of pamphlets previously published by the author; but these are so scattered as not to be always available—or, indeed, convenient for reference—to anyone desirous of studying the important question of Town Sanitation. In "Sanitary Engineering," these previous writings by Mr. Latham have been collated and published, with the addition of a considerable amount of further information and experience, which together form one of the most valuable publications we have met with upon this important subject. In dwelling upon the importance and necessity of sanitary measures, the whole community is appealed to in a way easy to be understood by the non-scientific world, whilst in dealing with the manner in which such measures can best be carried out, Mr. Latham's experience cannot fail to prove of considerable value to future labourers in the same field of engineering science. At various times and places, earth, air, fire, and water have respectively been advocated as the best means of disposing of fœcal matter from towns, but, from a comparison of London with other great centres of population, where different methods have been applied, it is argued that water-carriage "is the best adapted to the varied requirements of a town population for effecting the speedy removal of the principal matter liable to decomposition."

In carrying into effect the works necessary for the sewerage of a town, three distinct operations have to be performed, viz.:—(1) The drainage of the surface; (2) The drainage of the subsoil; and (3) The removal of fœcal and other liquid refuse. In these several operations, the mode of dealing with the rainfall of districts comes under the first heading, and this is comprehensively dealt with, having due regard for varying situations, the geological character and the physical outline of different districts. On the subject of sewers—their form and relative capacity—the information afforded is most complete, accompanied as it is by tables of discharge, &c. The course and sectional form of sewers is well explained; but, perhaps, the most valuable portion of the book for engineers is that which relates to their construction under varying circumstances and in passing through different geological formations, not the least important part of the subject being that which relates to the construction of sewers through sand and water-bearing strata. The flushing and ventilation of sewers occupies a considerable portion of the work, proportionate to its importance; whilst the concluding pages are devoted to the various forms of water-closets, &c., having special reference to the best means of connecting house-drainage with sewers. On the whole, this is a work of considerable merit, and the author has dealt with his subject in a clear and comprehensive manner, leaving little if anything to be desired.

Annual Record of Science and Industry for 1872. Edited by SPENCER F. BAIRD. New York: Harper, Bros. London: Sampson, Low, and Co.

THE second yearly volume of this work has just reached us. It reminds the reader, in some respects, of the well-known "Year-Book of Facts," but it takes a wider range, and enters more into detail. The "General Summary of Scientific and Industrial Progress during the Year," which is placed as an introduction, is carefully and fairly compiled, and gives a clear view of the course of scientific discovery during the past twelve months. The references to the various Transactions, journals, &c., quoted, are made on a novel and, we think, useful system. Each is designated by a letter and a number, the former signifying the country where it is published, and the latter the individual paper. An "Index to the References" gives the explanation. Thus, A 1 signifies the "Chemical News." The number of German scientific periodicals is significant, exceeding as it does that of the English and French taken together. The authorities quoted vary very much in value, and to some of the paragraphs the editor has prefixed a judicious "it is said." An extract from a German journal recommends the addition of *ammonia* to lessen the amount of sugar required in preserving acid fruits. An excess of ammonia, we are told, can be remedied by the introduction of a little vinegar. As the salts of ammonia. especially the acetate, have a decidedly nauseous flavour, we hope no lady will be persuaded to try this method.

Sanitary science is treated at great length. We notice paragraphs calling attention to the danger of using soaps made from putrescent animal matters, and extracts from Mr. Husson's paper on the milk of cows suffering from cattle-plague. A paragraph on the comparative value of antiseptics, taken from the "Academy," assigns to benzoic acid a higher power than to carbolic. "Metallic salts," such as sulphate of copper, occupy the highest place, whilst "inorganic salts," with the exception of bichromate of potash, "have but little power." These two statements seem scarcely reconcilable. General Scott's process for utilising, or, rather, for wasting sewage is described at length in an extract from the "English Mechanic." We also meet with the "recommendation" of the Rivers' Pollution Commissioners. We trust that this is a "last appearance" prior to its departure for that "limbo large and broad," of which Milton sings. There are many interesting paragraphs in this book to which we may recur on a future opportunity. The work further contains a list of eminent scientific men who have died within the year, and concludes with an elaborate index.

The appearance of this volume is a proof of the increasing demand for scientific literature on the other side of the Atlantic. It is not improbable that in the course of a few years America may occupy as prominent a position in chemical science and in chemical manufactures as she has already done in the mechanical arts.

Report on Béton Aggloméré; or Coignet-Béton, and the Materials of which it is Made. By O. A. Gillmore, Major Corps of Engineers, Brevet Major-General, U.S.A. Washington: Government Printing Office. 1873.

Practical Treatise on Limes, Hydraulic Cements, and Mortars By Q. A. Gillmore, A.M., Brigadier-General of U.S. Volunteers. Fourth Edition, revised and Enlarged. New York: D. Van Nostrand. 1873.

"Beton Aggloméré" is a term presumably unknown to the major portion of mankind; "Béton," however, under its synonym of "concrete," is to us dwellers in towns a well-known material. Béton aggloméré is a concrete of superior quality, an artificial stone, in fact, prepared under certain conditions from given materials. The conditions are the use of the best materials; the use of only sufficient water to convert the matrix of lime or cement into a stiff, viscous paste; the incorporation of the solid ingredients, as sand, with the matrix by a thorough or prolonged mixing or trituration, producing an artificial stone paste, incoherent in character until compacted by pressure, by which every grain of sand and gravel is completely coated with a thin film of the paste; and, finally, this béton, or artificial stone, is formed by thoroughly ramming the stone paste, in thin, successive layers, with iron-shod rammers. The materials employed in making this béton are sand, common fat, or hydraulic lime, or Portland cement. Having given a concise statement of particulars and conditions favourable and unfavourable to the formation and induration of this new artificial stone, General Gillmore proceeds to consider, in a lengthy course of actual experiment, its merit and demerit. This consideration is accompanied by a detailed description of the mortar-mixing mills, the proportion of materials employed in, and the tensile strength and other properties of the result of, the process. To complete the means of comparison a series of experiments and abstracts of experiments on Ransome's siliceous concrete, the Frear stone, the American building-block, the Sorel artificial stone, and Portland stone, are added. For the results, the reader—and even the general reader will derive much useful information from the work—be he builder, architect, or engineer, should refer to the actual description of the experiments. A sample of the work, however, may be epitomised from the chapter on artificial Portland cement, and its production by the English wet and the German dry processes. In the wet process we are told that the works in the vicinity of London employ both white and grey chalks of the neighbourhood, and clay procured from the shores of the Medway and the Thames. The clay and the chalk are mixed together in the proportion of about one to three by weight, and when a thorough mixture is effected in the wash-mill, the liquid, resembling whitewash in appearance, is conducted to large open reservoirs called bocks, where it is left to settle. When the raw cement mixture has attained the consistency of butter, it is shovelled out of the bocks, removed to stoves heated by flues, and dried. When dry, it is burnt with gas coke in perpetual kilns. The cement-clinkers formed during the burning are ground into the powder known commercially as cement. By the dry process, the chalk, or marl and clay are kiln-dried, mixed in suitable proportions, and reduced to powder. This powder is moulded into bricks from a stiff paste; the bricks are dried, burnt, and ground, as in the wet process. These processes, described in four or five pages, including the proportions giving best results, are supplemented by remarks which will be useful to every practical engineer. General Gillmore's work next quoted in our list is equally full of information, but deals with the class of hydraulic limes and cements only thus admitting more general detail. Both works are likely to be of much practical utility to the builder or engineer.

Mind and Body: the Theories of their Relation. By ALEXANDER BAIN, LL.D., Professor of Logic in the University of Aberdeen. Second Edition. Henry S. King and Co. 1873.

PROFESSOR BAIN is well known as the author of a work on Logic, and of various papers in the "Fortnightly Review," one of which, "On the Theories of the Soul," is printed as the concluding chapter of the volume before us. He is also known as a prominent member of the Rationalistic school. We fear the fact that this volume has reached a second edition in a very short space of time is a sign that Rationalistic literature is eagerly read in this country, and that the attitude of mind which it develops is largely on the increase.

The subject which Professor Bain discusses in this volume is the precise connection between the mind and the great nervous centre, the brain. He seeks to answer the question, "What has Mind to do with brain substance, white or grey? Can any facts or laws regarding the spirit of man be gained through a scrutiny of nerve fibres and nerve cells?" He reminds us, in the first place, of the very intimate connection existing between mind and body; the dependence of our frame of mind upon hunger, fatigue, bodily illness, stimulants, &c. "Bodily affliction," he remarks, "is often the cause of a total change in the moral nature." A little reflection will enable us to see that, although the mind is influenced more or less directly by other organs, the brain is the chief mental organ, and the mind is influenced through it. The brain is a very large and complicated organ; it is computed to receive no less than *one-fifth* of the entire amount of blood in the organism, which circulates through it, and this surely indicates some considerable activity. Physiologists have proved that the brain is indispensable to thought, feeling, and volition. Clever men usually have large brains, or brains with numerous and deep convolutions. Thus, while the average European brain weighs 49·5 ounces, that of Gauss weighed 52·6, De Morgan 52·75, Dr. Abercrombie 63, and Cuvier 64·5 ounces. We should like also to know the brain-weights of Descartes, Humboldt, Mozart, and Sir Walter Scott. Among idiots, brain-weights as low as 15, 13, and even 8·5 ounces have been found. An ordinary man, Professor Bain remarks, could not retain in his memory more than one-third or one-fourth, perhaps even less, of the facts remembered by Cuvier. "There would be no exaggeration in saying that while size of brain increases in arithmetical proportion, intellectual range increases in geometrical proportion."

An interesting account of the minute structure of the brain is given in the third chapter, from which we learn that the grey matter of the brain is composed of nerve-fibre mixed with small pear-shaped corpuscles, with prolongations to connect them with the nerves. The average diameter of the fibres is one six-thousandth of an inch, while the corpuscles range from one three-hundredth to one three-thousandth of an inch in diameter. The number of nerve-fibres constituting the optic nerve is very large, —probably there are as many as one hundred thousand. A few useful woodcuts accompany these descriptions.

In the next chapter we find, among other things, a physical theory of pleasure and pain, and in a long note (p. 75), we have the author's views regarding corporal and capital punishment. In place of hanging, he advocates an electric shock, and in place of flogging (which is revolting to the spectator, and inflicts permanent damage to the tissue), a succession of sufficiently severe magneto-electric shocks. The fifth chapter treats of the intellect, and at the outset destroys the long-existent idea that thought may be conducted in a region of pure spirit, unassociated with anything material. This is disproved by the fact that thought exhausts the nervous system, just as bodily exercise exhausts the muscles. Have we not even heard of people thinking themselves hungry? The "physical seat of ideas" is discussed in this same chapter, where we also find a physical treatment of memory, retention, or acquisition, which is defined as "the power of continuing in the mind impressions that are no longer stimulated by the original agent, and of recalling them at after-times by purely mental forces." Professor Bain explains memory as an effect produced by the continuation in a weaker form of the original impression which evoked the original nerve-current. Just as when we hear the last clang of a bell an after-impression of a feeble kind remains on the ear. But surely we cannot imagine, in the case of memory, that the nerve-currents are always flowing; if so, why is the effort of memory ever necessary? If so, again, have we not motion produced from nothing, or at least an original impulse producing indefinitely continuous impulses, after the manner of perpetual motion?

The book is full of sound logic; it is the work of an accurate and active mind. It is a physico-metaphysical treatise, and to the man of science sadly lacks the absoluteness of the experimental fact. It is most pleasurable to read this book, yet we confess that when we arrive at the last page we find ourselves just as wise (or rather, ignorant) as we were before, in regard to the nature of mind.

On the Origin and Metamorphoses of Insects. By Sir JOHN LUBBOCK, M.P., F.R.S. Illustrated. Macmillan and Co. London; 1874. 108 pp., crown 8vo.

SIR JOHN LUBBOCK is well known to the world as an archæologist and anthropologist, and perhaps less well as an entomologist. Yet he has contributed no less than *thirty-five* papers to the Royal Society, and to various magazines, on entomology during the last twenty years; and, as he is not yet forty, we perceive that he must have studied the subject at a very early age. His first paper, "On Labidocera," appeared in the "Annals and Magazine of Natural History" for 1853.

The little work before us embodies in a popular form many of the more interesting results of his observations condensed from the above-mentioned memoirs. The articles have already appeared in "Nature," and the work forms the second volume of the *Nature Series* of books, which Messrs. Macmillan are now publishing.

The main subjects discussed are the classification, origin, and the nature of the different metamorphoses of insects; various views are traced, from the old standard "Entomology" of Kirby and Spence, one of the Bridgewater treatises, to the more recent memoirs of Müller, Agassiz, and Packard. The intelligence of insects comes out in a remarkable light. Many of our readers will remember Sir John's tame wasp at a recent meeting of the British Association. He remarks "we are accustomed to class the anthropoid apes next to man in the scale of creation, but if we were to judge animals by their works, the chimpanzee and the gorilla must certainly give place to the bee and the ant." For example (p. 11), the larvæ of certain insects require animal food as soon as they are hatched, and the mother-insect consequently provides them with caterpillar and beetles, by burying them in a cell side by side with the unhatched larva. But here a difficulty arises: "if the Cerceris were to kill the beetle before placing it in the cell, it would decay, and the young larva, when hatched, would only find a mass of corruption. On the other hand, if the beetle were buried uninjured, in its struggles to escape it would be almost certain to destroy the egg." Look then at the wonderful, but diabolical, instinct of the creature. "The wasp has the instinct of stinging its prey in the centre of the nervous system, thus depriving it of motion, and let us hope of suffering, but not of life; consequently, when the young larva leaves the egg, it finds ready a sufficient store of wholesome food." A certain species of ants keeps *Aphides* in bondage, just as we do cows, for the sake of the honey-dew which they collect; a certain kind of red ant is indolent, and keeps black ants to do work for it. Once more, there is a kind of beetle which is blind and helpless usually found in ant's nests; the ants care for all their wants and nurse them tenderly. These things, and much more, of the lives of insects are told us in popular language in Sir John's book, which we recommend, not alone to the entomologist, but to the general reader.

Ozone and Antozone: their History and Nature. By C. B. Fox, M.D. London: J. and A. Churchill.

If, as Dr. Fox not unjustly remarks, "to the philosopher, the physician, the meteorologist, and the chemist there is perhaps no subject more attractive than that of ozone," it must be conceded that there are few subjects in experimental science more fraught with difficulties and involved in doubts. Since Schonbein first announced his discovery more than thirty years of research and observation have elapsed, yet the very existence of ozone is little more than generally conceded. As to the laws of its occurrence and distribution, its properties and functions, the means for its recognition, and its artificial production, there is a singular amount of discrepancy in the results of different observers.

As to antozone, it is still regarded by many chemists—in England at least—as little better than mythological. Such being the case, there is evidently room and need for a work like the present. Dr. Fox has undertaken the laborious task of giving a digest of the most important facts connected with ozone and antozone, comparing and, as far as practicable, harmonising, the views of former investigators. He endeavours to "point out the circumstances, and the manner in which, and the reason why, ozone is observed in the atmosphere," and finally gives the results of his own observations.

The concluding section, on the methods of observing and detecting ozone in the atmosphere in the air, is without doubt the most interesting and valuable. The author concludes that iodised litmus and simple iodide of potassium are the only tests that can be considered trustworthy. For the precautions to be observed in their employment we must refer to the book itself. We are bound to declare that, in our conviction, Dr. Fox has merited well at the hands of the scientific public for the elaborate and well-arranged digest of facts which he has placed at their disposal, and we join him in the hope that his labours will furnish a sound basis for future investigations.

Elementary Treatise on Physics, Experimental and Applied, for the Use of Colleges and Schools. Translated and Edited from Ganot's "Eléments de Physique." By E. ATKINSON, Ph.D., F.C.S., Professor of Experimental Science, Staff College, Sandhurst. Sixth Edition, revised and enlarged. London: Longmans and Co. 1873.

THE most prominent enlargements to this edition consist in further elucidation of the laws of the polarisation of light, a description of Gramme's continuous magneto-electric generator, and a further development of the theory of heat as recently advanced. We are glad also to notice an extension of fundamental formulæ. Nothing need be said in further praise of so eminently a standard work.

A Treatise of Medical Electricity, Theoretical and Practical; and its Use in the Treatment of Paralysis, Neuralgia, and other Diseases. By JULIUS ALTHAUS, M.D., M.R.C.P. Lond. Third Edition, enlarged and revised, with 147 illustrations. London: Longmans, Green, and Co. 1873.

IN religious dogma everyone knows the old saying that described orthodoxy as "my own peculiar doxy," and heterodoxy as "everybody else's doxy." But dogma (taken in its sense of enforced opinion) is not peculiar to religious discussion; the next elder science, that of medicine, has been full of it from the days of Æsculapius and Galen till now. Even now we are not free from the error of recording opinion instead of fact. And such a view is impressed upon all students of electro-medical science. The opinions held in contrast with the facts ascertained and stated without bias, are in overwhelming proportion. Indeed, the proportion is so great, that the influence of error is perceptible upon the minds of the public, who are too apt, perhaps, to regard electro-medical practitioners with not a little undue severity. It is then the duty of a scientific serial to uphold the careful chronicle of facts, and discourage the register of mere opinion, founded, if of any foundation, upon incomplete observation of fact. Thus it is our duty to commend to those who may be about to become interested in electro-medical science the perusal of Dr. Althaus's book, and to express congratulation upon the attaining of a third edition. But not only will the reader in search of medical knowledge find here what he requires, but the general electrician will find also complete record of observations as to the action of electricity upon physiologically organic substances.

A Treatise on Acoustics in Connection with Ventilation, and an Account of the Modern and Ancient Methods of Heating and Ventilation. By ALEXANDER SAELTZER, Architect. New York: D. Van Nostrand.

MR. SAELTZER has given his subject great attention, and this attention has led him to some highly original views upon the relation of the propagation of sound to the density of the air, and the propagation of sound waves across strata of different density. Given the air and the propagation of sound waves across strata of different density, the author shows how it is that the voice of a speaker, whether proceeding from pulpit, platform, or proscenium, may be so reflected by the surface of the different air-strata as to be inaudible or confused to the hearers above or below the strata in which he speaks. Mr. Saeltzer next recounts the experiments that have led him to adopt certain principles of ventilation as aids in developing the acoustical properties of public buildings, by breaking up the air-strata. The book should be read, not only by the architect, but by every professional man whose duty it is to speak in public.

Experimental Research on the Causes and Nature of Catarrhus Æstivus (Hay-Fever, or Hay-Asthma). By C. H. BLACKLEY, M.R.C.S. Eng. London: Baillière, Tindal, and Cox, King William Street, Strand.

Mr. Blackley may be fairly congratulated on having produced a work which is not merely a valuable contribution to our medical literature, but which will be read with interest by many scientific men not connected with the profession.

Long-Span Bridges. By B. BAKER, Assoc. Inst. C.E. Revised Edition. London: E. and F. N. Spon. 1873.

To the student or to the engineer who desires to investigate the comparative theoretical and practical advantages of the various adopted or projected types of bridge construction, whether including box-plate, lattice, or bowstring girders, ribs, or suspension, we cordially commend this little work, as readable as it is accurate. The work really includes two sections, relating respectively to long- and to short-span bridges, and is illustrated with diagrams of type form built upon the long-span system.

PROGRESS IN SCIENCE.

MINING.

STATISTICS of considerable interest in connection with the coal-produce of this country have been published during the past quarter. The official returns furnished by the twelve Government Inspectors to the Home Office show that in 1872 there were 3016 collieries at work in Great Britain. In the previous year the number of mines was 3100. But in 1871 the number of coal-miners employed reached 418,088, whilst in 1871 the colliers numbered only 370,881. It appears that during 1872 the amount of coal raised was 123,393,853 tons, compared with 117,439,251 tons during the preceding year. This gives a decrease in the amount of coal raised per man; for whilst the average quantity gotten by each miner in 1872 was 295 tons, it amounted to 316 tons per man in 1871. As to the accidents, we note an increase in their number, but a slight decrease in the resulting deaths. Thus, in 1872 there occurred 894 separate fatal accidents, resulting in the loss of 1060 lives; in 1871 there were only 826 similar casualties, but they represented a sacrifice of 1075 lives. As usual only a small proportion of the deaths could be traced to explosions of fire-damp; indeed, out of the 1060 deaths in 1872 only 154 were due to such explosions.

It is not long since we called attention to some investigations on the connection between colliery explosions and the state of the weather, undertaken by Mr. R. H. Scott, F.R.S., of the Meteorological Office, and Mr. W. Galloway, who is now Assistant-Inspector of Mines in Scotland. These observers have since extended their researches, and have recently published an analysis of the catalogue of accidents which occurred during the year 1871. On the whole there were 207 explosions of fire-damp, of which 52 were attended with fatal consequences. It is believed that 113 of these explosions, or 55 per cent may be traced to the low state of the barometer: and that 39, or 19 per cent, are referable to a rise of temperature; whilst the remainder are unexplained by either of these meteorological causes. The paper contains some valuable instructions respecting the method of recording barometric and thermometric observations; these, of course, deserve careful study by the miner, especially now that the Coal Mines' Regulation Act of 1872 requires that "after dangerous gas has been found in any mine, a barometer and thermometer shall be placed above ground in a conspicuous position near the entrance to the mine." With regard to atmospheric pressure, the authors remark that if the barometer, after having remained nearly stationary for several days, descend half an inch or an inch during the next two or three days, the miner may expect to find fire-damp in greater quantity than usual in cavities in the roof and in the higher parts of the workings, and it may also appear where it had not been previously detected. If the temperature rise to 55° or upwards, the ventilating current is likely to be retarded. A sudden fall of the barometer—an inch in twenty-four hours or so—or a further fall after it has been unusually low for a day or two, points to the possible escape of gas, and calls for increased ventilating power, especially if the diminution of pressure be coupled with a rise of temperature.

We may observe that Mr. Galloway obtained the first of the prizes offered by Mr. Hermon, M.P., for Essays on the Prevention of Colliery Accidents. This Essay, which has recently been published in the "Mining Journal," is worthy of attentive study by those who have charge of our coal-mines.

Coal-cutting machinery received a fair share of attention in the Mechanical Section of the British Association at the late Meeting. Mr. Firth, of Leeds, well known for his zeal in promoting the use of such machines, read a paper "On the Introduction of Working Coal-Cutting Machinery in Mines." In this communication he described in detail his own form of coal-cutter, worked by compressed air; and during the meeting he gave many of the members an opportunity of seeing it at work in his pits at West Ardsley. Mr. Firth's machine has already been described in this Journal, and we have also noticed his efforts to promote the introduction of coal-getting machinery by offering some time ago a premium for the best form of machine.

At the same meeting, Dr. W. J. Clapp described the "Universal Coal-Cutting Machine," used at Nant-y-glo; this machine is driven by compressed air, but has the action of a borer rather than that of a pick. Mr. J. Plant, of Leicester, read a paper "On the Burleigh Rock Drill," and two examples of the drill were actively at work in the yard of the Church Institute, close to the room occupied by the Mechanical Section.

A new form of safety-lamp has been described before the North of England Institute of Mining Engineers, by Mr. Emerson Bainbridge, of Sheffield. The light is protected by a cylinder of glass below and of brass above, so that wire-gauge is scarely used in its construction.

"The Application of Compressed Air to Underground Haulage" was the title of a paper lately read before the Dudley Institute of Mining Engineers, by Mr. A. J. Stevens, of Newport, Monmouthshire. After comparing the cost of using steam-power underground with that of applying the steam to the compression of air, he described a small and apparently simple winding-engine, which he has recently patented, with the view of dispensing with animal power for underground haulage.

Deposits of coal, almost unrivalled in magnitude, are described by Baron Von Richtofen as existing in certain provinces in China. Whilst in the maritime provinces the coal-measures have been in some places completely removed by denudation, and in others only imperfectly preserved, some of the inland provinces are singularly favoured, and exhibit fine exposures of their coal-bearing rocks. The province of Shansi contains at least 30,000 square miles of coal-producing ground, occupied partly by a fine bituminous coal and partly by anthracite. Some of the coal-seams are said to attain a thickness of 30 feet. In fact China offers to the miner coal-fields which are said to rival, if not to surpass, even those of the United States.

A description of the iron-ores of the Bidasoa, in the Pyrenees, by Dr. Röhrig and Mr. Haas, has appeared in a recent number of the "Chemical News." The mineral seems to be chiefly spathose ore, or carbonate of iron, superficially changed by atmospheric influences into a brown iron ore, or hydrous peroxide. From the published analyses the ores appear to contain more or less manganese; and, as no phosphorus is recorded, they would probably produce a pig-iron remarkably well adapted for conversion into Bessemer steel. The ores occur in highly-inclined lodes, coursing for the most part through syenite, though some of the lodes are found in adjacent Silurian rocks and others in Triassic limestones.

In the Island of Anglesey a valuable deposit of copper-ore has long been worked at the Parys Mountain, but it is a disputed point whether this represents the only great treasury of mineral wealth in that island, or whether copper-lodes sufficiently rich to be worked are not widely distributed. It consequently becomes a matter of interest to notice any local vestiges of old copper workings in this island. The Hon. W. O. Stanley has communicated to the Royal Archæological Institute some interesting notes on this subject. It is hardly to be doubted that copper was exported from Anglesey prior to the landing of the Romans, and, indeed, the mineral wealth of Mona may have attracted the Romans thither. About the year 1640 a cake, or *massa*, of smelted copper was discovered, bearing the inscription "Socio Romæ." It is said to have been found at Caerhên, the ancient Conovium. In 1840 a second cake was discovered in Llangwyllog, and in 1869 three cakes were found at Castellor. Two years later three cakes were dug up at Bryndon, Amlwch, and passed into the hands of Mr. T. F. Evans, who has described them in a recent number of "Iron." One of the cakes is remarkable for being stamped, on two hunches, with the letters I V L S. They are curious relics, which may have been smelted for use in making the bronze weapons and implements known to have been used at an early epoch of civilisation.

METALLURGY.

Some valuable researches on alloys, especially on those of copper and tin, have been conducted at the Paris Mint by M. Alfred Riche, who has recently published his results in the "Annales de Chimie et de Physique." The fusibility of the alloys was determined by means of Beçquerel's thermo-electric pyrometer, with platinum and palladium wires, constructed by Ruhmkorff. The author finds that most alloys of copper and tin suffer liquation at the moment of solidification, and hence it is almost impossible to obtain their true melting-points. Exceptions, however, are furnished by those alloys whose atomic constitution corresponds to the formulæ $SnCu_3$ and $SnCu_4$; these alloys are not liquated, and the former of them possesses peculiar properties, differing in colour, for example, from all others of this class. Tempering increases the density of bronzes rich in tin, and annealing diminishes the density of tempered bronze. Whilst steel is hardened by being suddenly cooled, bronze is softened by this treatment. Yet the bronze is not sufficiently soft to be readily worked; and it has long been a vexed question how the Chinese manage to work their tam-tams and other instruments of bronze. Experiments made many years ago at the Ecole des Arts et Métiers, at Châlons, showed that such objects might, though with much trouble, be wrought in cold-tempered bronze; but the process was too difficult to be employed industrially. It was afterwards asserted by St. Julien that the Chinese worked their bronze at a red heat; yet it was difficult to understand this, since bronze is extremely brittle at high temperatures. M. Riche appears to have settled the difficulty by showing that although bronze cannot be readily worked either cold or at a bright red heat, yet it is easily manipulated at intermediate temperatures. Taking advantage of this fact, M. Riche and M. Champion have succeeded in imitating the Chinese tam-tams.

With respect to unalloyed copper, Riche finds that its density when alternately submitted to mechanical treatment, tempering and annealing, is variously affected according as the metal is protected from or exposed to access of air; in the former case the mechanical action increases, and in the latter case diminishes the density. The introduction of a small proportion of iron gives considerable tenacity and hardness to copper. The author has also studied a number of alloys of copper and zinc.

Within the last year or two considerable attention has been directed to the so-called "Phosphor-Bronze," an alloy of copper and tin, with more or less phosphorus, according to the purposes for which it is intended. Messrs. Montefiore-Levi and Künzel have brought this alloy to great perfection, and have applied it to a great variety of industrial uses, whilst in this country it has been prominently brought forward by the Phosphor-Bronze Company. When the proportion of phosphorus is large the alloy is extremely fluid, and therefore well adapted for castings; whilst its fine colour and close texture recommend it for decorative work. Certain forms of the bronze are characterised by great ductility and malleability; and may be readily rolled, drawn, or embossed; whilst other varieties are as hard as soft steel, and may be worked into tools and cutting instruments for use in gunpowder mills. It has been much recommended for ordnance and small arms; whilst the miner has used it for the wire ropes of his winding machinery, and the iron-smelter for the tuyeres of his blast-furnace. Numerous experiments on the mechanical properties of phosphor-bronze have been made at Berlin and Vienna, and by Mr. Kirkaldy in this country.

A detailed account of some experiments on Swedish steel, conducted by Mr. Kirkaldy, at his Testing Works at Southwark, has lately been published. The specimens tested were manufactured at the Fagersta Works by Mr. Christian Aspelin, and have been exhibited at Vienna. The enquiry was directed to the behaviour of the steel under the action of tensile, compressive, transverse, twisting, and shearing stresses; in fact, to all such strains as actually occur in engineering works.

A new mode of tempering steel has been suggested by M. Caron. His plan is to plunge the red-hot metal into water heated to about 55° C., and he also proposes to restore "burnt iron" by a similar process.

The nature and uses of modern steel formed the text of Mr. Barlow's address as President of the Mechanical Section at Bradford; and although it naturally dealt chiefly with mechanical questions, it is yet deserving of the metallurgist's attention. To the same section Mr. Joseph Wilcock contributed a paper on the Bowling Iron Works at Bradford, in which he traced the history of iron-making in this district, and gave an excellent description of these extensive works. The Bowling and the Low Moor Iron Works, both celebrated for the manufacture of boiler-plates, were visited by many of the members of the British Association.

Sir Francis C. Knowles has recently laid before the Society of Arts a description of his method of refining and converting cast-iron into either malleable iron or steel. His object is to effect the conversion without the waste of heat and material which attends puddling and other converting processes now in use. Sir Francis employs as his source of heat the combustion of gases rich in carbonic oxide, and mixed in due proportion with atmospheric air heated to 500° C. Having melted the pig-iron in a cupola with coke or anthracite, he collects the gases, frees them if necessary from carbonic anhydride, enriches them by addition of free carbonic oxide, and is thus enabled to produce by their combustion a temperature of 2500° C. If higher heat and greater rapidity be required, he uses the cupola-gases for heating his retorts, and employs pure carbonic oxide specially generated, and thus obtains a temperature of 2979° C. In either case the mixture of carbonic oxide and air is blown into the molten metal, thus producing carbonic anhydride and nitrogen at a high temperature; but this heat may be readily utilised, whilst the products of combustion are passed through kilns or retorts, containing anthracite or coke, and the carbonic anhydride is thus reduced to carbonic oxide, which is again available for combustion. The finery or converter used in this process is peculiarly constructed, with the view of withstanding the high temperature to which it is subjected, and the interior is lined with a mixture of protoxide of iron, manganese, emery, bauxite, and caustic soda; a highly basic preparation is thus obtained for the lining, as it is considered desirable that the cinder should also be basic, not containing more than 30 per cent of silica. To eliminate the sulphur and phosphorus, caustic soda and rich oxide of iron or manganese are employed; and when superior iron and steel are to be prepared the use of nitrate of soda or permanganate of soda is recommended. It will thus be seen that, so far as the oxidising agents are concerned, the process is only a modification of the Heaton process.

MINERALOGY.

Two distinct minerals are known in jewellery as *Cat's-Eye;* the one being an opalescent quartz enclosing asbestiform fibres, which, lying in parallel directions, give the appearance of a fibrous structure to the quartz; whilst the other mineral, sometimes called for distinction sake, "oriental cat's-eye," is a fibrous chrysoberyl. Some very fine cat's-eyes have been received within the last year or two from the Cape of Good Hope. These stones are generally of a rich brown colour, but occasionally blue, red, and white. Some authorities have regarded the Cape cat's-eye as a form of crocidolite, whilst others have supposed that it is only a coloured fibrous quartz; but its true character appears to have been determined by Dr. Wibel, of Hamburg. After an attentive study of the chemical and microscopical properties of the mineral in question, he comes to the conclusion that it is not strictly fibrous quartz, but a pseudomorph of quartz after fibrous crocidolite. He shows that the so-called brown fibrous quartz, originally described by Klaproth, is merely a mixture of pure white quartz with goethite, or hydrous peroxide of iron;

whilst the blue variety is essentially a mixture of white quartz and crocidolite. The fibrous character is, in both cases, due to the replacement of fibrous crocidolite by quartz; the brown variety being the product of a perfect and slow alteration, whilst the blue is the result of an imperfect and rapid alteration.

Among the ejections from the eruption of Vesuvius in 1872, there occurred certain minute crystals, which were described by Scacchi as a new species under the name of *Microsommite*. This species has recently received attentive study by Vom Rath, who, in spite of the unusually small size of the crystals, has had the patience and acuteness to examine them crystallographically. About 1500 of these little crystals, weighing together only $\frac{1}{10}$th of a gramme, have passed through his hands. The crystals are colourless hexagonal prisms, striated vertically, and terminated by dull basal planes. The hardness is about equal to that of felspar; and the specific gravity is 2·6. An analysis of the very small quantity at Vom Rath's disposal yielded:—Silica, 33; alumina, 29; lime, 11·2; potash, 11·5; soda, 8·7; chlorine, 9·1; sulphuric acid, 1·7. The following expression is given as the probable formula:—

$$(\tfrac{2}{5}K_2O,\tfrac{3}{5}CaO),Al_2O_3,2SiO_2+NaCl+\tfrac{1}{13}CaO.SO_3.$$

The origin of microsommite may be traced to the reaction of steam, charged with chloride of sodium, on the volcanic minerals leucite and augite.

Under the name of *Horbachite* a new ore of nickel has been described by Dr. Knop, of Carlsruhe. In the neighbourhood of Horbach, near St. Blasien, in the Black Forest, there occurs a deposit of this ore, which has at different times invited exploration. The ore has been described as a nickeliferous magnetic pyrites, but analyses of the pure mineral show that it is really a distinct species. The mean of several analyses gives:—Sulphur, 45·87; iron, 41·96; nickel, 11·98. From this result we may deduce the formula $4Fe_2S_3+Ni_2S_3$. Knop calls attention to this association of sulphides as unique, no metallic sesquisulphides having previously been known to occur in a native state.

Some recent observations by Dr. Schrauf, of Vienna, show that *Brookite*, one of the three native forms of oxide of titanium, must be referred to the oblique or monoclinic system. The crystals affect, however, a decidedly prismatic habit; hence the reason why previous observers have placed this species in the prismatic system. Schrauf recognises three distinct types of crystal in brookite.

Prof. Rammelsberg has laid before the German Geological Society some papers on mineral arsenides and sulphides. In one of these he discusses the mutual relations and the chemical constitution of the compounds of arsenic and antimony which occur native. He is led to regard the metallic arsenides not as true chemical compounds, like the sulphides, but as isomorphous mixtures of metal and arsenic; whilst the sulpharsenides are mixtures of these with bisulphides. What he says with respect to the compounds of arsenic applies also to those of antimony.

Some beautiful crystals of *Torbernite*, or uranium-mica, have been recently raised in Cornwall, we believe from the neighbourhood of Redruth. They are notable for exhibiting an unusual development of the pyramid, so that the crystals are very different in general appearance from the common tabular forms.

A preliminary description of the meteoric stone which fell, some years ago, at the Barratta Station, near Deniliquin, in Australia, has recently been published by Mr. Archibald Liversidge, of the University of Sydney.

Nefediewite is a new Russian mineral, described by Pusirewsky. It seems to be an amorphous substance, resembling lithomarge.

ENGINEERING—CIVIL AND MECHANICAL.

Harbour Works.—On the 25th November last, Mr. L. F. Vernon Harcourt read a paper before the Institution of Civil Engineers on the construction and maintenance of Braye Bay Harbour, Alderney. This harbour was designed by the late Mr. James Walker, C.E., and the works in connection with its construction were commenced in 1847. The Admiralty intended, in the first instance, to make only a small harbour, but subsequently gave directions for the enlargement of the scheme. In 1856, the design, then in course of construction, consisted of a harbour of 150 acres, with a depth of water of 3 fathoms and upwards, sheltered to the west and east by two breakwaters. The western breakwater, about 4700 feet in length, had been constructed, but the eastern breakwater was abandoned; and the harbour was consequently exposed to winds blowing from any quarter between N.N.E. and E.S.E. The western breakwater was exposed to the whole force of the Atlantic, and the effect of the fury of the storms was increased at Alderney by the rapidity of the tides near the island, occasioned by a peculiar confluence of currents in the bay of St. Malo. The breakwater was constructed on the "*pierres perdues*" system—a mound of rubble-stone being deposited in the line of the proposed work from hopper barges towed out by steam-tugs. As soon as the mound was sufficiently consolidated, it was surmounted by the superstructure, consisting of a sea wall and of a harbour wall 14 feet and 12 feet thick respectively, flooded at first at the level of low water, and built without mortar, the intermediate spaces being filled up with rubble, the batter of the sea wall being 9 inches and of the harbour wall 4 inches to 1 foot. To protect the lower or quay level, a promenade wall, 14 feet high, was built on the seaside, consisting of two masonry walls set in mortar, with filling between. In 1860, when the superstructure had been carried out 2700 feet from the shore, the design was somewhat modified. The breakwater was narrowed by reducing the width of the quay to 20 feet, the batter on the sea face was altered to 4 inches in a foot, solid masonry was substituted for the concreted hearting, and the foundations of the harbour wall were commenced at the same level as the sea wall. The head was built in 1864. The foundations were laid 24 feet below low water level, across the whole width of the breakwater. The first nine courses, each 3 feet thick, consisted of concrete blocks faced with granite headers; the upper portion was built of masonry in cement. The most exposed face stones were joggled and dowelled together, and several of the corner quoins were further secured by iron bars and diagonal straps. Two red leading lights on the shore mark the entrance to the harbour at night. The cost of the works of construction and maintenance to 1872 amounted to £1,274,200, of which £57,200 was expended in repairs.

Water Supply.—With a view to improving the water supply of Paris, the Montsouris Reservoir is now in course of construction to receive the waters of the Vanne. It occupies an area of 54,000 square metres, or 13½ English acres, and will contain 300,000 cubic metres, or 66,000,000 gallons of water, being the amount of three days of the normal flow of the canal which supplies it. The entire work is constructed of stone and cement, the exterior walls being strengthened by oblique arches having a thickness of 3 metres. The bottom is level except at the approaches to the wall, where is a series of sumpts of little depth, separated by partitions in order to form a number of arches which support the interior gallery. In front of each of the recesses thus formed are pillars, carried up to support the vaults of the arches which form the top of the building. The river Vanne is expected to afford daily a supply of about 100,000 cubic metres, or 22,000,000 gallons of excellent water. Hitherto the daily supply of water to Paris per head, as a mean, has been but 24 gallons; with the addition of the Vanne water, this will be raised to about 34 gallons.

Sewage.—The great development of the industries of the town of Rheims, and its consequent increase of population, having led to the necessity of providing facilities for the disposal of the sewage, a commission was appointed to investigate the subject, and it was decided, in 1870, to establish on a large scale a system of chemical purification, and a direct application of the sewage to agricultural purposes. Two processes have been tried—purification direct by chemical means, and irrigation. The Suvern agent, composed of chloride of magnesia, of lime, and of tar, was tried without effect, and the application of sulphate of alumina was equally unsuccessful. The latter process did, however, produce a certain effect, but an imperfect one, and that at an enormous cost. The most successful trial was that made with a mixture proposed by

MM. Houzeau and Devédeix, composed of a combination of lime and aluminous lignites, which latter are found in abundance in the vicinity of Rheims. MM. Houzeau and Devédeix's process was subjected to an extensive trial, and 2,500,000 cubic feet were thus treated in 1872. The cost for the purifying mixture per cubic foot was very trifling, and from the experience gained from the experiments, it was found that for treating the whole of the sewage waters of Rheims in this manner, the outlay would amount to £7000 a year, besides the interest on capital expended; and the operation would produce about 33,000 tons of manure of an inferior value. Although, then, this agent of MM. Houzeau and Devédeix purified the water well, but at a high cost, the process could be considered in certain special cases as a complement to irrigation; but taken alone, it was evidently too costly. Irrigation was next tried, and, in consequence of the results obtained, the Sanitary Commission has recommended the acquisition of about 450 acres of suitable land. The estimated cost of works, &c., is £40,000, and the annual cost of maintenance and working about £3800.

Boilers.—At the recent Vienna Exhibition, M. J. C. C. Meyn exhibited two of his patent high pressure boilers. This boiler consists externally of two cylinders, of which the upper is the smaller. The furnace is half internal and half external, and requires but little building. There is no bridge to the grate, which communicates directly through a short vertical flue with a central combustion-chamber. This chamber is traversed by 76 flattened vertical water-tubes, which form one of the principal features of the boiler. They are wrought-iron welded tubes, with horizontal channels across their sides. Through these tubes the water circulates from the lower part of the boiler, and between them the flame must pass. From the roof of the combustion-chamber a double ring of tubes leads up to the upper part of the lower shell, the upper cylindrical shell being of such a diameter that it stands inside these rings of tubes. This upper shell is enclosed in a smoke-box of the same diameter as the lower shell, and made of sheet iron. The ordinary water-level is two-thirds up the height of the upper tubes, and the upper shell, therefore, serves as a steam dome of large capacity, and has apparently been really effectual in preventing priming. The steam is led away from the top of the boiler through a pipe which forms a triple ring round it in the smoke-box before it is allowed to escape.

A paper on the "Strength of Boiler Shells" has recently appeared in the columns of the "Nautical Magazine," in which it is stated that the Board of Trade Surveyors have all along recognised as a fundamental principle of their practice the following opinion, expressed by Sir William Fairbairn, in 1854, viz.:—"Steam boilers of every description should be constructed of sufficient strength to resist eight times the working pressure, and no boiler should be worked, under any circumstances whatever, unless provided with at least two—I prefer three—sufficiently capacious safety-valves." It appears the "Galloway's rule" is most common amongst these surveyors, the *rationale* of which is that in the absence of tests witnessed by an officer of the Board of Trade, the strength of iron is assumed to be 48,000 lbs. per square inch in plates and rivets; this includes the effect of friction at the joints, and supposes the holes to be drilled, the strain to be applied lengthwise to the plate, and the rivets to be Lowmoor, or equal to that in quality. When the rivets are subjected to double shear, or where the strain is applied crosswise to the plate, only 43,000 lbs. is allowed as the strength per square inch of the rivet, or of the plate respectively. The section of the shell is taken as the length of the boiler by the thickness of the plate; but practically, the length of the section will be greater than the length of the boiler on account of the doubling of the plates at the circumferential seams. When the double rivetting is zigzag, as it should always be in boiler shells, the section is increased about 7 per cent by this extra material. To give effect to this, the 48,000 lbs. was increased about 7 per cent, or to 51,520 lbs., or 23 tons. A great deal of very valuable information and calculations are given in this paper, relative to the proper strength of boiler shells, but space will not admit of our following out the subject further at present.

Railways.—A new line of railway 15½ miles in length was opened early in September between Bristol and Redstock. It joins the Great Western Railway at Bristol, and is laid on the narrow gauge system. At present only a single line of rails has been laid, but the arches are wide enough for a double line if necessary at any future time.

The remaining portion of the Devon and Somerset Railway, between Wiveliscombe and Barnstaple, 36 miles in length, has recently been completed. Owing to the line running at right angles to the principal valleys and water-courses of the district, the works are in some places very heavy, involving, in addition to deep cuttings and high embankments, several tunnels passing through the ridges between the valleys, and some large river viaducts. One of these, crossing the valley of the river Tone, near Wiveliscombe, is 110 feet high, and has four spans, each 100 of feet. The construction adopted being lattice girders carried on stone abutments and piers. Another viaduct across the valley of the river Bray, in Castle Hill Park, is 100 feet high, and has six spans, each of 100 feet. The construction being similar to that adapted for the Tone valley viaduct; and there is also a large iron bridge over the river Exe, near Dulverton.

The list deposited this year at the Private Bill Office of projects for consideration during the coming session embraces 395 notices, of which, however, only 244 are accompanied by plans. Amongst these 121 relate to railway schemes, 7 to tramways, and 65 bills belong to the miscellaneous class, which includes docks, harbours, gas and water-works, reclamation schemes, street extensions, and local improvements. Amongst the railway projects are the following:—A proposition to complete the Inner Circle of the Metropolitan Railway by means of a line from Aldgate to the Metropolitan District Railway at Cannon Street. The construction of a Metropolitan Inner Circle Completion, and Eastern Extension Railway, by which it is proposed to form a line from the Metropolitan District Railway in Queen Victoria Street to the Metropolitan Railway at Aldgate, with extensions to Mile End and Bow, and junctions with the North London and East London Railways. It also includes a new street from King William Street to Fenchurch Street, and the widening of the latter street. Besides these there are the following schemes affecting the Metropolis, viz., the Metropolitan and South-Western Junction Railway Company's project for a line in Fulham between the authorised Hammersmith Extension Railway at North End-Road, and their own authorised line at a point near where it crosses the river; the Wandsworth, Fulham, and Metropolitan Railway scheme for a line from the Wandsworth Bridge to the authorised Metropolitan and South-Western Junction Railway at Fulham; the Ealing, Acton, and City Railway, which will unite the Hammersmith and City Railway with the Great Western and Brentford line; and the Acton and Hammersmith Railway to unite the North and South-Western Junction and Hammersmith Extension Railways. There are several projects for improved communication with the Crystal Palace; and most of the principal lines of railway propose improvements in the shape of short lines and junctions whereby increased facilities will be afforded to the travelling public.

GEOLOGY.

Physical Geology.—Baron Von Richthofen has recently described the extensive sheet of mud-like strata which extends over Northern China. It is called "Loess," from its resemblance to the river-loam of Germany, and has a thickness varying from 1000 to 2000 feet. There is not a trace of this formation in Southern China, and its occurrence in the northern parts produces quite a different class of physical scenery. The expanse of loess is cut up by vast chasms, a thousand feet in depth, along whose bottoms the streams flow. As regards its origin, after showing that it extended from the sea-level to an altitude of 12,000 feet, the Baron stated that it must have been formed where it is now seen, and by sub-aërial agencies. Of these, one of the chief had been the wind and the fine dust-storms, which often lasted for many days together. Rains also were great agents in the accumulation. He had examined hundreds of sections of the loess, and had never found any fresh-water shells, but myriads of uninjured land shells.

Mr. W. T. Blanford has brought forward evidence of glacial action in Tropical India in early geological times. Some peculiar beds, possibly of carboniferous age, consisting of large boulders, embedded in a fine silt, were considered by him as evidence of glacial action; the deposition of the boulders being due to the agency of ice.

Professor Von Baer has recently written a memoir on the Caspian Sea, to which Dr. Carpenter has drawn attention. Instead of the Caspian being intensely salt, like the Dead Sea, its waters had only about one-third of the usual saltness of ordinary sea-water. This was due to the precipitation of the salt in the lateral lagoons, where great beds were forming. These salt pans drained off the saline substances, and thus left the water pure.

Professor Herschel and Mr. G. A. Lebour have made some experiments upon the conducting power for heat of certain rocks. Of all the rocks experimented upon, shale resisted heat the most, whilst the metamorphic rocks were the best conductors. The experiments showed that the rates of conductivity of heat through rock-masses depended on the structure of the latter. A thick bed of shale would arrest it. Hence, denudation of rocks, if carried on to any great degree, would alter the quantity of heat conducted from the interior of the earth to the surface. The experiments threw considerable light on underground temperatures.

The Rev. O. Fisher, in a note on the "Origin of the Estuary of the Fleet, in Dorsetshire," expresses his opinion that it is the eastern half of a submerged valley, similar to, though on a larger scale than, the one which now forms the Weymouth Backwater, its former western side having been encroached upon and destroyed by the waves of the Weat Bay.

Mr. G. H. Kinahan has lately described the Water Basin of Lough Derg, in Ireland, and discussed the method of its formation. He points out that the bays and all wide stretches across the basin conform with the strike of lines of breaks or displacements in the adjoining country, while the minor features of the coast lines are due to the weathering along minor breaks, joint systems, or lines of bedding. It seems that in this basin all the changes in the bearings of the different lines of deeps are connected with the strikes of the different faults in Slieve Bernagh and Slieve Aughta, and that the islands, rocks, and shallows are on the upthrow sides of these lines of faults.

It appears, from a despatch of the British Vice-Consul at Rhodes, that a volcanic outburst has taken place in the island of Nissiros, one of the Sporades, in which there existed a volcano supposed to be extinct. Shortly before the 10th June, 1873, new craters opened in this volcano, and from them ashes, stones, and lava were ejected; many fissures, from which hot water flowed, were produced in the mountain, and the island was daily shaken by violent earthquakes. From Rhodes, at a distance of about 50 miles, the smoke rising from the new craters could be seen.

Stratigraphical Geology.—Mr. W. Whitaker (assisted by J. B. Jordan) has prepared a large block-model of London and its neighbourhood on a scale of 6 inches to 1 mile, coloured to show the geology of the area, which includes Hampstead, Wimbledon, Great Ilford, and Shooter's Hill. All the superficial deposits are represented, and the subterranean geology to a depth of 1100 feet is shown on the sides of the model, which is divided into nine pieces. This model is placed in the museum at Jermyn Street.

Perhaps the Sub-Wealden Exploration is the most important topic in British geological circles. Commenced in 1872, the depth at present reached is a little over 300 feet. The boring commences about 230 feet down in the known Purbeck Beds, the thickness of which previously known in Sussex was somewhat over 300 feet; according to Mr. Topley, about 230 additional feet of strata have been made known by the boring, and in this series are some valuable beds of gypsum. Professor Phillips has communicated the latest intelligence in regard to the boring, stating that the lowest beds now reached are of marine origin, that a specimen of *Lingula ovalis*, a shell of the Kimeridge clay, has been examined by him, so that the beds reached are already below the Purbeck. He remarks that the great upper clays of the oolites have been touched without encountering shore sands or shelly oolites,—no Portlandian rocks have appeared. Clay deposits may be found for a considerable depth; there may be no triassic beds, and he thinks that the palæozoic rocks may be reached at no enormous depth, and with no unusual difficulty.

Mr. Whitaker has recorded the discovery of Thanet Sand on the northern outcrop of the London Basin, near Sudbury, where it had not previously been observed.

Palæontology.—Dr. Von Mojsisovics has published the first part of his geological investigation of the neighbourhood of Hallstatt, in which he describes the remains of *Cephalopoda* obtained from the Zlambach and Hallstatt beds. He indicates repeatedly, especially in connection with the genera *Lytoceras*, *Pinacoceras*, *Sageceras*, and *Arcestes*, that he can by no means arrange the *Goniatites* as a distinct generic series in opposition to the *Ammonites*. The triassic representatives of the above-mentioned genera are most closely related in all essential peculiarities of organisation and habit to Goniatitic predecessors; the *Ammonites* from the Permian sand-stone of Artinsk, Waagen's Permian *Ammonite* from the salt range of the Punjab, and certain triassic forms partially bridge over the gap which still exists between the older *Goniatites* and the *Ammonites* of the Trias. By far the greater part of the genera of *Ammonites* occurring in the Alpine Trias have their roots in the Palæozoic *Goniatites*; and some of them may apparently even be traced back into the Upper Silurian formations. The greater part of these Palæozoic genera become extinct in the Upper Trias, when they attain the height of their development, but at the same time show signs of senile degradation, analogous to the phenomena observed during the gradual extinction of the later Ammonitic types in the Cretaceous period.

Professor W. H. Flower has described a new species of *Halitherium* from the Red Crag of Suffolk. It is of especial interest as furnishing the first recorded evidence of the existence in Britain of animals belonging to the order Sirenia. This new species presents many characters common to the Manati and the Dugong.

PHYSICS.

MICROSCOPY.—A new freezing microtome, for facilitating the cutting of thin sections of soft tissues, has been contrived by Dr. William Rutherford, Professor of Physiology in King's College, London. The construction of the machine will be easily understood by reference to the cuts. Fig. 2: A, hole in the brass plate (B); C, tube; D, screw; E, indicator; F, screw for fixing the machine to a table; G, box for holding the freezing mixture; H, tube for permitting the water to flow from the melting ice. Fig. 3: Vertical section. The hole A is shown containing a piece of tissue, and the box G containing the freezing mixture; K, a movable bottom to the hole A; R, transverse section of the knife used in making the sections: the other letters the same as in Fig. 2. The tissue to be frozen and cut is placed in the tube (A). The section is made, as in the ordinary instrument for cutting wood sections, by gliding a knife horizontally through the tissue projecting above the level of the plate (B). The thickness of the section is regulated by the indicator (E). The machine may be employed for two objects:—For cutting tissues hardened in the ordinary way, by chromic acid, &c.; and second, for cutting tissues hardened by freezing. The second method of using the machine will be more readily comprehended after a description of the first, which is simply this:—Place a portion of hardened tissue in the hole A, and pour around it a mixture of paraffin (5 parts) and hog's lard (1 part), melted by the aid of a gentle heat. Or the paraffin mixture may be first poured into the hole, and the piece of tissue afterwards introduced, and held in any desired position, by means of forceps, until the paraffin becomes sufficiently hard. In order that the paraffin may fairly support the tissue, it is necessary that the surface of the latter be dry. This is easily accomplished by leaving it exposed to the air for some time, either with or without previous immersion in spirit. When the machine is used for the second object—that is, for freezing—the following directions are to be attended to:—Surround the freezing-box with two or three layers of flannel, and screw the machine to a table. Unscrew the movable bottom, or

FIG. 2.

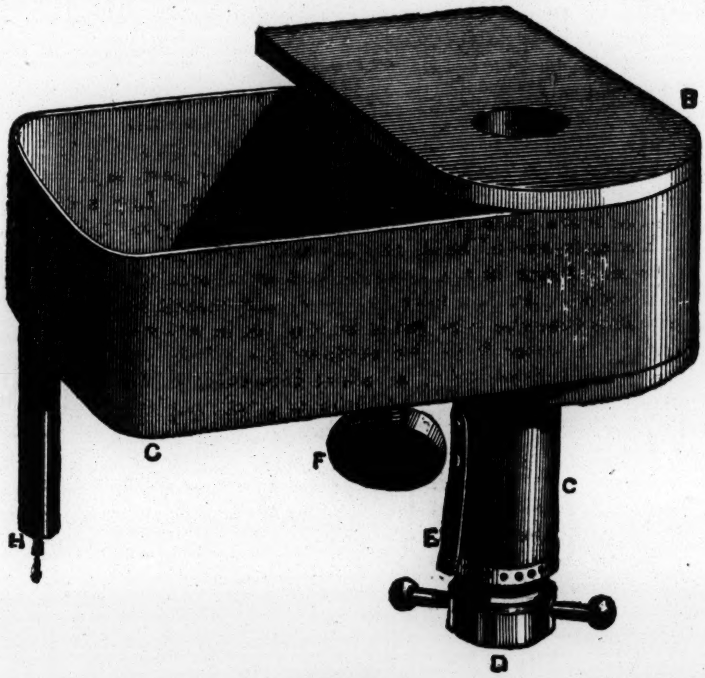
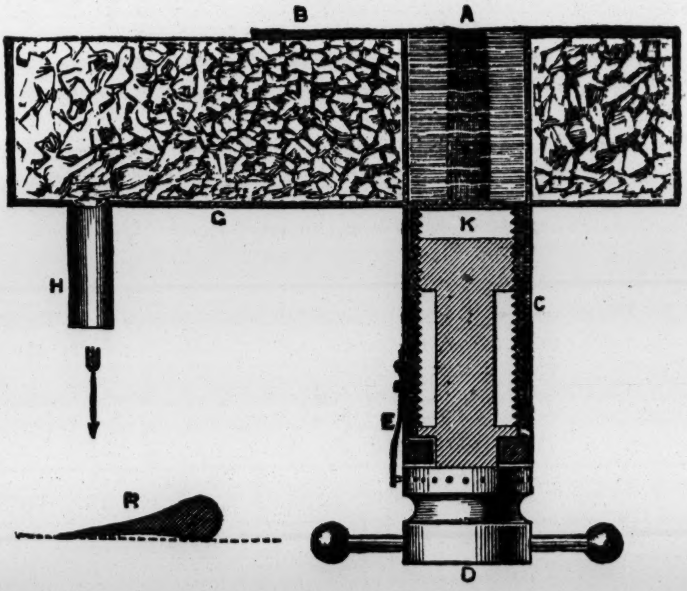


FIG. 3.



plug (K, Fig. 3), and pour methylated spirit into the tube (C); oil the side of the plug; replace it, and screw it down to any desirable extent, and there leave it. The object of this is to prevent the screw from becoming fixed by the freezing. The spirit which has come above the plug (K) must be thoroughly removed by means of a towel, and the small slit at the margin of the plug carefully closed by means of hog's lard, which should be spread in a thin layer around the entire margin of the plug, to prevent the spirit from in any way reaching the cavity above the plug. The screw (D) must not be touched until after the freezing is completed, in case this accident occur. The tissue to be frozen, together with an imbedding fluid, are placed in the hole. For ordinary purposes a solution of gum arabic may be employed, prepared in the following manner:—Add to 10 ounces of water, 2 drachms of camphorated spirit and 5 ounces of picked gum arabic; when the gum has dissolved, strain the fluid through calico, and preserve for use in a corked bottle. The gum when frozen can be sliced as easily as a piece of cheese. The gum or other fluid should be first placed in the hole of the machine, and when a film of ice has formed at the periphery the tissue should be introduced and held against the advancing ice until it becomes partially frozen. In this way a portion of tissue may be secured, in any position, for the process of section. Lay a piece of gutta-percha upon the brass plate (B) so as to cover the cavity containing the tissues and prevent the entrance of heat, and the accidental entrance of salt from the freezing mixture. Place in the freezing-box (G) alternate quantities of finely powdered ice and of salt, and take care they are pushed round the tube of the machine, and also that the tube (H) is kept open, in order to permit of the constant egress of the water from the melting ice. The freezing can be most rapidly effected by the addition, at short intervals, of ice and salt, and by repeatedly stirring the mixture, in order that the escape of water may be facilitated. It is possible, especially in winter, to have the tissue frozen too hard to permit of its being readily cut. It splinters when it is too hard. This is prevented by discontinuing the further addition of the freezing mixture, or by dropping water or a three-quarter per cent salt solution, at the ordinary temperature, on the surface of the frozen tissue, or by heating the razor slightly. With regard to the cutting tool, it may be stated that a razor answers perfectly well for all ordinary purposes. The blade should always be *hollow* on both surfaces (R, Fig. 3). It is a mistake to employ a flat knife, for it is scarcely possible to keep the surface of the brass table of the machine smooth enough to permit of the knife lying quite flat. The knife should be *pushed* obliquely through the tissue, which should be cut at one sweep. This is not possible with a frozen tissue, if the ice be too hard. For an unfrozen tissue embedded in the paraffin the knife should be wetted with methylated spirit. In the case of freezing it is not necessary to wet the knife, for the melting ice does so readily. The machine has been made by Mr. Baker, of High Holborn.

Mr. John Barrow* recommends naphthalin as a material for embedding soft tissues for the purpose of cutting sections. The advantages claimed for naphthalin over wax and other substances are—A low fusing-point, absence of contraction, the minimum of injury to the edge of the knife, and very ready solubility after cutting in benzol or spirit, so that the substance is at once removed from the section without injury.

Mr. F. H. Wenham, at the October meeting of the Royal Microscopical Society, exhibited, under the microscope, a roughed pattern on a piece of glass produced by the American "sand-blast," remarking that the microscopical appearance of the "greyed" surface is quite distinct from that of ordinary ground glass. Observations under a low power give some insight into the *modus operandi.* It was stated, at the late meeting of the British Association, in the discussion that followed the description of the process, that a large crystal of corundum was speedily perforated with ordinary sea-sand and a blast-pressure of 300 lbs. per square inch. Corundum is several degrees beyond emery in hardness, approaching near to that of the diamond. But it was further stated that, under the conditions named, diamond itself became speedily worn away. At first sight it seems extraordinary that the hardest known material should quickly be destroyed by one considerably softer. The microscope indicates that this is caused by the force and velocity of impact; it is not a grinding process at all, but a battering action, similar to that of leaden bullets against a block of granite. A polished glass surface exposed for an instant to the sand-blast shows an aggregation of points of impact, from which scales of fractured glass have broken away in an irregular radial direction. It appears as if a pellet of glass had been driven in by the collision of the sand, and the wedge-like action thus set up had driven away the surrounding glass. All these spots or indentations, when tested by the polariscope, show a coloured halo round each, proving that the glass surface is under strain and ready to yield to further fracture. The action therefore is not so much due to the hardness of the striking particles as to the force and velocity of impact. This is sufficiently great to destroy the cohesion of the surface of the material operated upon. The external layer is carried against the under stratum, and the material is crushed and disintegrated by a portion of its own body.

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* Manchester Literary and Philosophical Society, Microscopical and Natural History Section, April 21, 1873.

Mr. William Webb, whose minute writings on glass are so well known to microscopists, disputed the reality of the finer bands of lines on Nobert's test-plate, on the ground of the tool cutting away the surface of the glass, and leaving, as he showed by certain diagramatic sections, irregular jagged furrows. Dr. Woodward, of the Army Medical Museum, Washington, in reply, reminds the readers of Mr. Webb's paper, that there is a physical reason which compels us to believe that the first fifteen bands, at least, of the nineteen-band plate are composed of real and distinct lines, and that the distance apart of these lines must approximate very closely to what was intended by Nobert. When the bands of Nobert's plate are illuminated by oblique light, and are looked upon from above with a low power (too low to show any of the lines), each band appears as a smooth coloured stripe. From the known wave-length of the colour seen, and the angle of the incident pencil, the distance which the lines of any band must actually be apart can be computed by the well-known formula for the spectrum of gratings enunciated by Frauenhofer, and the distance thus obtained agrees with that at which Nobert ruled the lines. On the other hand, the angle of the incident pencil being known, and Nobert's given distance being assumed to be true, a table of wave-lengths for the different colours may be calculated, and the wave-lengths thus deduced agree substantially with those computed by other means. Nobert has discussed the whole matter in the 58th volume of "Poggendorff's Annalen" (1852). His discussion leaves, in the opinion of Dr. Woodward, no room for the possibility of a doubt of the objective reality of the lines up to the fifteenth band. Dr. Woodward calls attention to the fact that this reason is altogether independent of our ability to resolve the lines with the microscope. In fact, it enabled Nobert to know that his plates were correctly ruled long before the resolution of any but the coarsest bands had been effected. As no spectral colour is obtained in the bands finer than the fifteenth, the formula of Frauenhofer cannot be applied to them. In fact, the formula demonstrates that if these bands are actually ruled, as claimed, they can give no spectral colour. Dr. Woodward has no hesitation in expressing the opinion that the four higher bands (sixteenth, seventeenth, eighteenth, and nineteenth) have also an objective reality. He bases this opinion upon the comparison of their optical appearances, as seen with the best glasses, with the appearances of the lower bands (especially those from the ninth to the fifteenth). These appearances are quite the same in both cases, and, as similar results follow similar causes, he infers the existence of real lines in the four higher bands, since he knows they exist in the others. Dr. Woodward has recently examined two new test-plates by Nobert,—the first ruled for Professor Barnard, of Columbia College, the second for the Army Medical Museum,—in which the maker has attempted to rule lines twice as fine as those of the nineteenth band. These plates have twenty bands. The first ten correspond respectively to the first, third, fifth, seventh, ninth, eleventh, thirteenth, fifteenth, seventeenth, and nineteenth of the old plate. The lines in the second group of ten bands purport to be ruled at the following distances apart:—The eleventh band, $\frac{1}{11000}$th of a Paris line; the twelfth band, $\frac{1}{12000}$th; and so on up to the twentieth band, lines of which are said to be $\frac{1}{30000}$th of a Paris line apart. These new bands have not at present been resolved.

Mr. J. J. Monteiro, in a letter to the "Chemical News,"* gives some account of the habits of the "plantain eaters" (*Turacus albo-cristatus*), the birds from the feathers of which the colouring matter turacin was extracted by Professor Church, and which is distinguished by its remarkable two-banded spectrum. These birds are common on the West Coast of Africa, and in the part known to the writer, viz., from Loango, in 5° S. lat., to Fish Bay, in 15° S. lat., they abound. Over the whole country mentioned, and for a considerable distance inland, copper is found, most abundantly distributed, as malachite or green carbonate; specks of the green mineral are everywhere to be found. Mr. Monteiro considers it highly probable that the birds swallow small particles of the cupreous substance with the gravel, &c., so commonly taken by all birds. This may account for the large quantity of copper discovered by Professor Church in the feathers of these birds. Mr. Sidney Lupton also remarks† that two green love birds (*Melopsittacus undulatus*) were in the habit of pecking at the bars of their cage or any brass-work accessible to them. The feathers of these birds also yielded traces of copper. The spectrum of the green feathers had not been examined. The lower band of the turacin spectrum (Fig. 5) corresponds in position, although not in extent, with the band of ruby glass (Fig. 4): extent of absorption, however, is in many cases dependent upon the intensity of the coloured medium, many bands, as those of the aniline dyes, being easily made to absorb to a greater or less extent, according to the amount of dilution or the thickness of coloured medium through which the light is allowed to pass. (The bands between the Frauenhofer lines, extending half-way across the upper spectrum, are those of nitrate of didymium, and, from their sharpness and convenient distribution, are convenient in mapping spectra by artificial light).

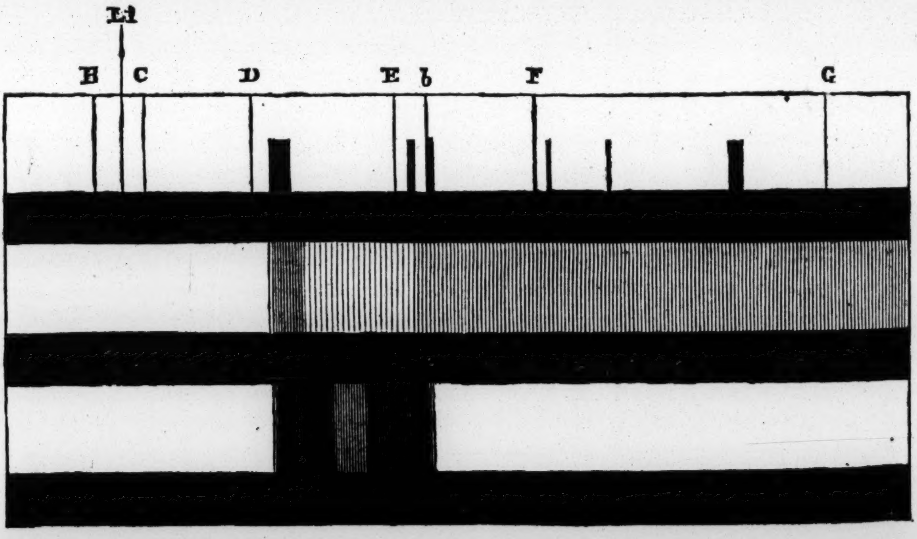

Fig. 4.

Fig. 5.

Electricity.—Mr. Casselberry, of St. Louis, has been experimenting for several years with voltameters. He finds as the result that two voltametric apparatus (of the ordinary kind) connected together, not in series, but with their oxygen and hydrogen electrodes attached by wires, the oxygen electrodes to the positive, and the hydrogen electrodes to the negative pole of a battery, that there is then generated in *each* voltameter an equal quantity of gas to that generated in only one voltameter with the same battery. The experiment is borne out by the law of derived circuits, the second voltameter being in effect a derived electrolytic circuit to the first. Should the experiments (and they are most carefully recorded) stand the test of general experience, the sequence of great importance in the practical deposition of metals would appear to follow naturally and immediately.

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* "Chemical News," vol. xxviii., p. 201.
† *Ibid*, vol. xxviii., p. 212.

The experiments were repeated with a magneto-electric machine, with results precisely similar to those obtained with the battery.

The electric current has again been utilised for the purposes of ascertaining longitude, this time of Harvard observatory. The results show the difference of time between Harvard and Greenwich to be for the following years:—

	Hrs.	Mins.	Secs.
1867	4	44	31·00
1870	4	44	31·05
1872	4	44	30·99

The mean of which is 4 hrs. 44 mins. 31·01 secs. The close agreement of these results indicates a very near approximation to the truth, and this fundamental longitude may now be considered as settled. Adding to the above value the difference in time between the Washington and Harvard observatories, 23 mins. 12·12 secs., we get for Washington 5 hrs. 8 mins. 12·12 secs.

In America interesting lectures upon the system of fire, burglar, and other alarms as laid down by the American District Telegraph have been delivered. Particulars will be found in the "Journal of the Franklin Institute."

In natural science electricity has again made progress. Dr. Burdon-Sanderson has investigated the electrical phenomena which accompany the contractions of the leaf of *Dionæa muscipula*, and he has demonstrated their collateral character with those of nerve and muscle.

INVESTIGATION OF THE FLUORESCENT AND ABSORPTION SPECTRA OF THE URANIUM SALTS.—President Morton and Dr. Bolton, after briefly noticing the labours of their predecessors in this department of research, describe the methods employed, which do not differ from those of Stokes and Becquerel, except in small details, and the means at their disposal for securing accuracy in the measurements. A variety of uranium compounds were specially prepared, and their spectra, both of fluorescence and absorption, were carefully mapped out and measured. Illustrations are given showing some of the most characteristic of these spectra. Very characteristic differences were found between the spectra of certain salts, and in a number of cases one body can be readily discriminated from another by this means. Indeed, in this investigation impurities in many commercial compounds of uranium were thus detected, and, in other cases, the progress and consummation of a change in composition, or in the formation of a compound, was watched and recognised with the greatest ease and precision. In almost every case there is a tendency of the light to fade off in the bands towards one side more gradually than towards the other. In nearly all spectra this graduation is greatest towards the less refrangible end of the spectrum. The character of any one band is, as a rule, a type of all the bands of a spectrum; but to this a remarkable exception is found in the double acetates of uranium generally, and especially in the sodium salt whose fluorescence is the brightest. In the spectrum of this salt the first four bands at the lower end of the spectrum in the orange and red differ entirely in character and spacing from the rest, except the fifth, which seems to be in a transition state. It is also true, as a rule, that double salts with the same acid have bands of a like character; but to this also there are decided exceptions, and it is by no means true that all salts with the same acid have like bands. This chances to be true in the case of the sulphate and normal double sulphates, but in the case of the acetate and double acetates, fluoride and double fluorides, chloride and double chlorides, as also among the numerous hydrates of the sulphates, it fails to maintain itself. Nothing could well be more unlike than the spectra of uranic oxychloride and the potassium chloride. The question arises, how far the spectra of substances are constant, and in what way a change of spectra is to be interpreted. The authors find that no substance has its spectrum changed by anything which does not affect its composition, excluding the effect of heat, and certain cases in which a substance has been caused to give a continuous spectrum in place of its normal one. We may, therefore, ascertain in many cases whether under certain treatment a body has or has not suffered a change in composition, and trace such a change step by step.

Absorption Spectra.—There are in the uranium salts two sorts of absorption—one directly related to their fluorescence, and the consequence of the fact that those rays which excite fluorescence must themselves disappear, their motion taking that other form; and the other an absorption having no immediate relation to fluorescence, but representing rays of the spectrum whose motions are converted into heat or some other form of force not sensible to the eye (*Phil. Trans.*, 1852, p. 520). Absorptions of the first class are best studied by direct observation, combined with a process closely allied to that described by Stokes as his *third method*, which consists in throwing a pure spectrum upon a screen of the substance in question, or upon the vertical side of a tank containing a solution. With the solid screen, the location of general maxima of fluorescence will correspond with maxima of absorption, and with the tank the absorption can be directly seen as embodied in dark blades or triangular masses of shade running into the tank (as seen from above) from the side away from the light. These appearances will often indicate the existence and relative intensity of absorptions, whose exact locations we can measure by examining the transmitted light directly with the spectroscope. The spectra of absorptions not directly related to fluorescence are, as a rule, best studied by transmitted light. The difference between different salts as regards their apsorption-bands is very great; and, while in many cases solution has a vast effect upon fluorescence, it sometimes produces but little effect upon the absorption-bands. In other instances, however, very marked changes occur, and, when these are followed out to their legitimate conclusions, they lead to some very remarkable results. Thus, if we examine the absorption-spectra of the uranic acetate and the various double acetates, we shall find that in the solid state they present great variety in the exact location of the bands, but in solution we have exactly the same spectrum for all. The conclusion therefore presents itself, that in solution all are reduced to the same state, which could, of course, only be by the breaking up of all the double salts. Indeed, from this, supported as it is by other observations, we do not hesitate to conclude that *no double acetate can exist in solution in water*, but must break up into its two single salts. Nor do our conclusions stop here, but we must reserve others until some of the facts on which they are founded have been described. A similar experience leads us to a like conclusion in the case of the sulphates, oxychlorides, &c. Attention was drawn to the fact of such displacement by one of us in September of last year, but its true bearing has only been perceived recently, since a large number of observations have been accumulated. A change of character, rather than of position, produced by solution in the absorption-spectrum of didymium sulphate, was observed by Bunsen in 1866 (see "Pogg. Ann.," vol. cxxxviii., p. 100, and "Phil. Mag.," 4th series, vol. xxxii., p. 181). The position of the band of uranium nitrate, while unaffected by solution in water, is notably changed by other solvents, as the following table will show:—

Bands.	1.	2.	3.	4.	5.	6.
Glycerin	87·6	96·0	107·8	123·0	136·0	148·6
Water	89·8	98·5	108·5	118·7	129·5	142·0
Alcohol	—	99·4	111·7	—	—	—
Hydrochloric ether..	—	99·4	110·8	123·7	—	—
Ether	—	100·0	112·6	123·0	—	—
Acetic ether	—	101·0	111·7	128·0	135·4	—

Effects of Heat.—It was observed by Stokes that canary glass and the nitrate of uranium had their fluorescence reduced by heating, and that at a temperature much below redness their influence upon light in this respect was quite suspended. In the solutions of nitrate of uranium all fluorescence was extinguished near 212° F. The substance regained its fluorescence on cooling. He also remarked that no such action appeared in fluorescent vegetable solutions. Gladstone finds, as a rule, the effect of heat is equivalent to a concentration of the solution, and amounts to an increase in the amount of absorption. In a paper "On the Change of Colour produced by Heat in Certain Chemical Compounds," E. J. Houston pointed out the curious and novel fact, that, in all cases where no chemical change was involved, solutions, as well as solids, changed to tints lower in the spectrum on the application of heat. The loss of fluorescence in a few substances when heated appears to extend (with certain limitations) to all the uranium compounds, both in their solid state and in solution. We find that in the case of the anhydrous ammonio-uranic sulphate, fluorescence is sensibly diminished at 140° C., and is almost destroyed at 260° C. The hydrate does not show any marked loss of fluorescence below the point at which it begins to part with its water. The same is true of the potassium sulphate. The sodio-uranic acetate is much more sensitive. Experiments were made with it and other salts. It was observed that at about 50° C. the brightness of the fluorescence was reduced, and that the uppermost decided band (81·8) lost its distinctness. At about 116° C. it seemed to reach a minimum. At that temperature the uppermost band had vanished, and the lower ones were too faint for measurement. No further change was noticed on carrying the temperature to 150°. Solutions are still more sensitive. The authors next enter upon a detailed examination of various uranium compounds, from which we select the following passages:—

Uranic Acetate (*normal*), $U_2O_3C_4H_3O_3+2HO$.—This substance fluoresces very brightly, different specimens, however, differing, probably from the presence of minute traces of foreign matter. Its solution yields a very bright fluorescence, which is reduced by the addition of a trace of alcohol, ether, glucose, or sucrose, and is destroyed by a very small amount of hydrochloric acid. The fluorescent light of the solution yields a continuous spectrum.

FIG. 7.

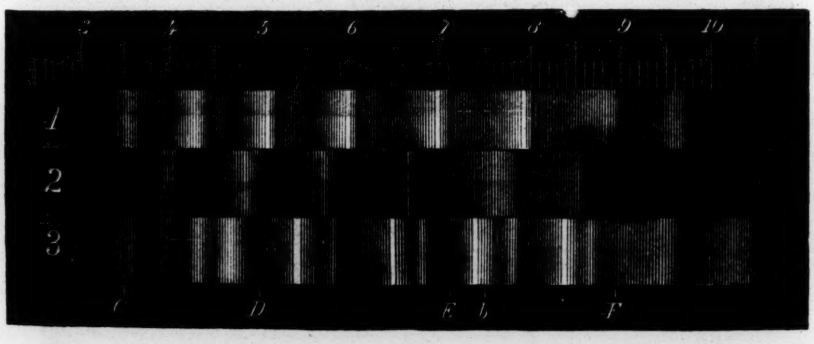
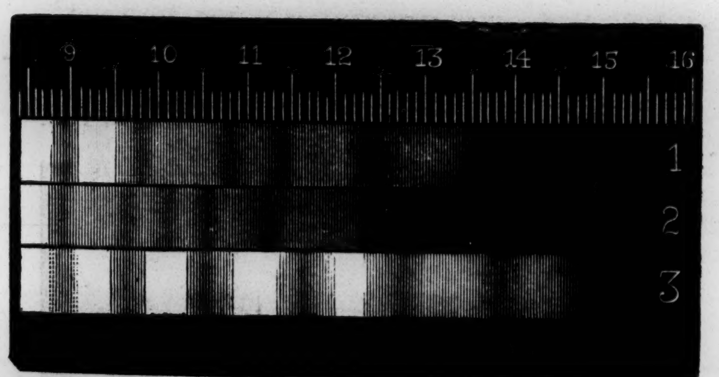


FIG. 8.



When examined with the spectroscope its fluorescent light emitted by the solid yields a system of eight bands. Of these the 1st and 7th are very faint, and the 8th only appreciable with a strong light and wide opening to the slit of the spectroscope. The brighter bands show a very abrupt termination

FIG. 9.

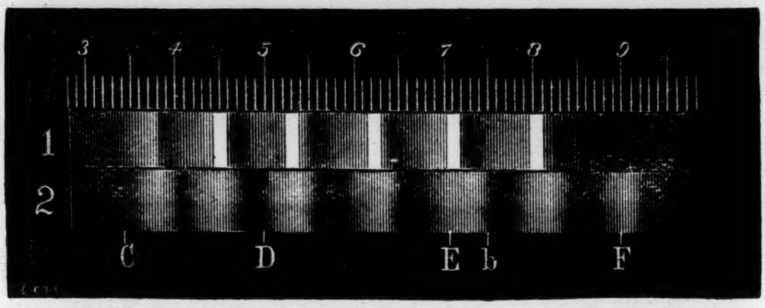


FIG. 10.

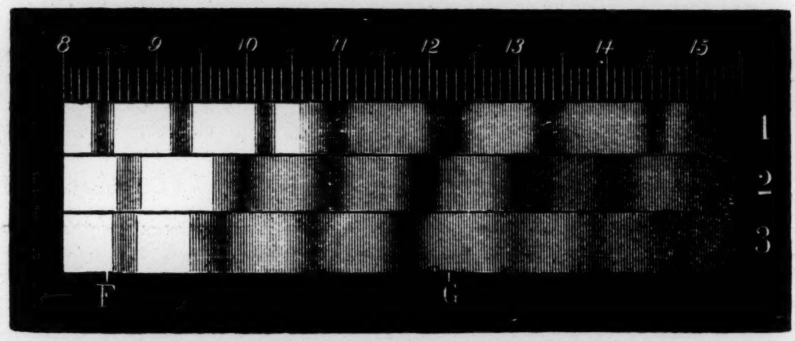


FIG. 11.

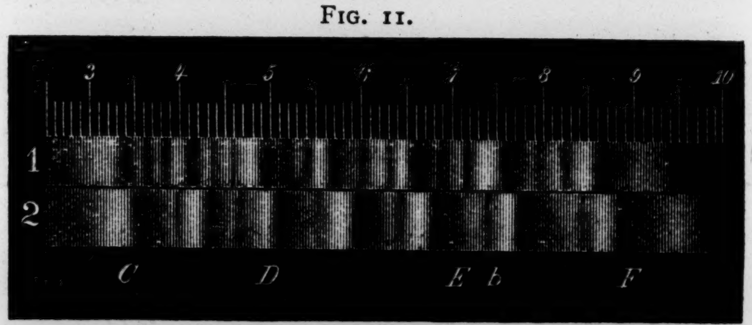
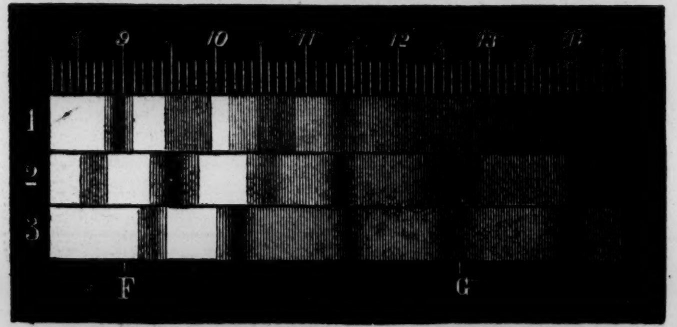
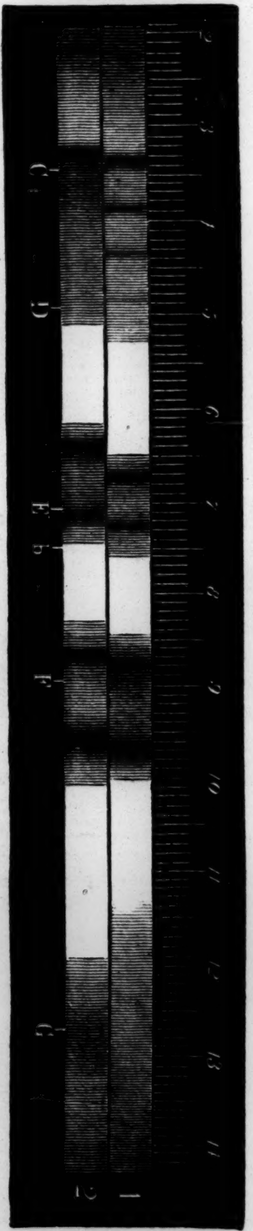


FIG. 12.



towards the more refrangible end, and shade off gradually so as to look like pieces of moulding illuminated from the violet side of the spectrum. No. 1 in Fig. 7 will give an idea of this, as well as of the positions of the various bands. By turning the spectroscope obliquely on the bottle containing this salt, an absorption-band at 107 can be distinguished with ease, but none above this can be made out, and, as regards this matter, the strongest contrast exists between the uranic acetates and its double salts. By crushing a few grains to a fine powder, with a little water, between slips of glass, we may observe the absorption-bands by transmitted light with facility, and get a spectrum of a curious character as regards the irregular spacing of its bands. No. 1 of Fig. 8 will give a good idea of this. Another noteworthy point is the very strong general absorption, which almost obliterates details of the spectrum, and makes it impossible to recognise any bands above one at about 135. In solution this general absorption is increased, and the absorption-bands are blended so that little can be done in the way of measuring them. If we make a solution of the neutral acetate in water, and examine its absorption, we shall find a faint band at about 105, and some indication of one at about 117, but a very heavy general absorption over the entire region above the first-named band obliterating all variations of shade. The addition of a little acetic or other strong acid will, however (while destroying the fluorescence of the solution), clear up its absorption-spectrum in a remarkable manner, giving us such a one as is shown at No. 3 of Fig. 8, which we have reasons for regarding as the absorption-spectrum of *the double acetate of uranium and water*.

FIG. 13.



Anhydrous Uranic Acetate, $U_2O_3C_4H_3O_3$.—If this normal uranic acetate is dried, at a temperature of 100° C., for some hours, it becomes opaque, and of a lighter and purer yellow tint, and is found to yield a fluorescent-spectrum, the bands of which are like those of the normal salt, but are all displaced downward in the spectrum. No. 2 of Fig. 7 will show the arrangement of these bands. The brightness of the fluorescence is very much reduced, and the first and last bands could not be made out with the apparatus at present in use. By inclosing this substance in dry powder, between slips of glass, its absorption-spectrum was observed with transmitted light, and is shown at No. 2 of Fig. 8. The general absorption is greater in this than in the case of the normal uranic acetate.

The Double Acetates.—These bodies give spectra which, whilst differing remarkably from that of the single acetate of uranium, agree strikingly among themselves. The fluorescent spectrum of the sodio-acetate of uranium is a type of a perfect double acetate of spectrum. All the double acetates show their absorption spectra with very great ease. The arseniates appear peculiarly fixed and inflexible in the relations under review. The four compounds examined exhibit but one spectrum of fluorescence and one of absorption. The characteristics of the former are seen in Fig. 9, No. 1, and those of the latter in Fig. 10, No. 1. The double carbonates of uranium fluoresce faintly, showing a character like that of the less brilliant double acetates. See

Fig. 9, No. 2. The neutral oxychloride has a very great general absorption, and shows some bands, but is almost devoid of fluorescence. The uranic oxyfluoride gives a spectrum generally resembling the acetate, normal sulphate, &c., and shown in Fig. 11, No. 2. The absorption-spectrum is likewise well marked, and is shown in Fig. 12, No. 2. When dissolved in water and acidulated with hydrofluoric acid, it yields the absorption-spectrum No. 3 of Fig. 12. Of the double oxyfluorides the potassium salt is the most brilliant, and is represented in Fig. 11, No. 1, its absorption-spectrum being shown in Fig. 12, No. 1. Of the fluorides examined none show any fluorescence, but their absorption-spectra are well worthy of notice. That of the uranous salt is shown in Fig. 13, No. 2, and that of the potassio salt in Fig. 13, No. 1. Uranic formiate gives no fluorescence, and shows an absorption-spectrum with faint bands. The uranic nitrate fluoresces brilliantly, yielding what may be called the normal uranic spectrum. It shows an absorption-spectrum of well-marked, regular bands. All the oxalates yield absorption-spectra, which are very well marked, and seem identical, carrying out the idea above suggested as to the breaking up of double salts when dissolved. The phosphates, like the arseniates, show a remarkable fixity of spectrum, but the absorption-spectra present a variety of forms. The absorption-bands of the neutral and acid sulphates are shown in Fig. 14, Nos. 1 and 2, and in 3 we find the bands of any other uranic sulphate as yet examined. It would seem with the sulphates as with the acetates that all are reduced to the same condition when in solution. Fig. 15 shows the absorption-spectra of the following double sulphates:—(1) ammonio-uranic sulphate; (2) ammonio-diuranic sulphate; (3) magnesio-uranic sulphate; (4) rubidio-uranic sulphate; (5) sodio-uranic sulphate; (6) thallio-uranic sulphate. For a detailed description of the characteristics of the various spectra here mentioned the reader is referred to

FIG. 14.

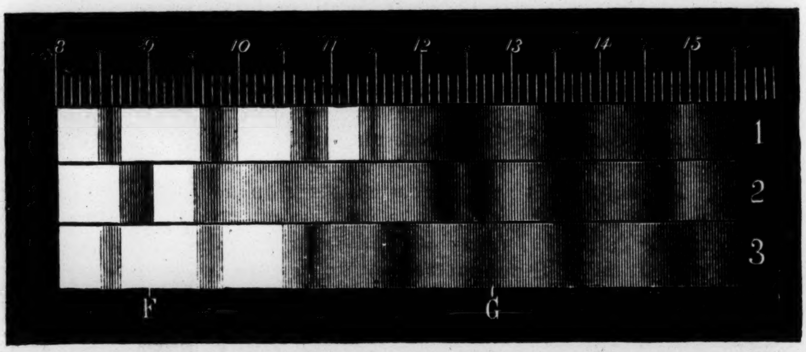
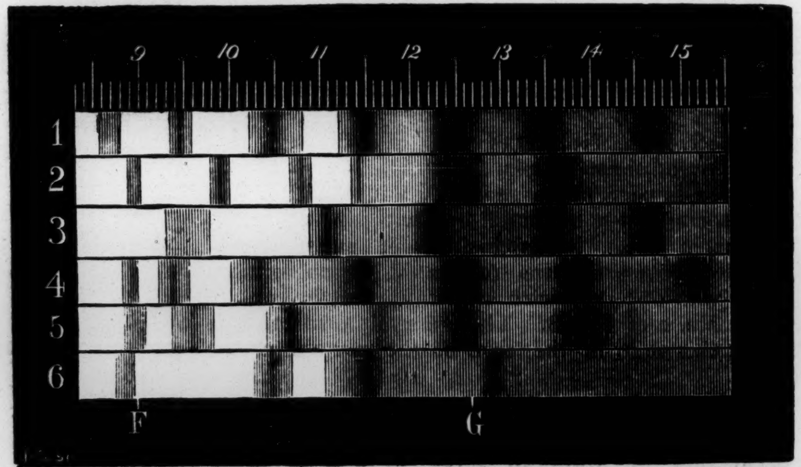


FIG. 15.

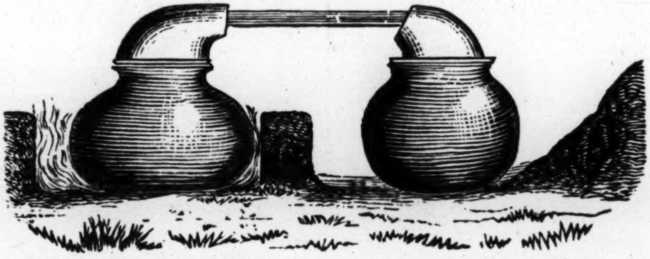


the papers as communicated by the authors to the "Chemical News," and which must be regarded as a valuable contribution to chemical and physical science.

TECHNOLOGY.

An instructive paper by Capt. G. A. Strover, of Mandalay, on the Metals and Minerals of Upper Burmah, appeared in the "Chemical News" of October 10. Gold, silver, copper, iron, lead, tin, platinum, graphite, coal, jade and amber, sulphur, saltpetre, rubies, sapphires, garnets, salt, petroleum, india-rubber—all seem to abound. The sulphur is found in efflorescent salts, and is manufactured from metallic sulphurets. The mode of extraction is illustrated below in Fig. 15. Common chatty-shaped vessels are made on the spot from the soft blue clay in which the ore is found. The larger vessel is filled with broken ore, and placed on a fire, a clay retort being fitted to the top, and communicating with the smaller vessel. The sulphur is thus sublimed and condensed as shown, after which the retort is broken, and a hollow tube of flowers of sulphur extracted therefrom which is superior to that condensed in the vessel.

FIG. 15.



The following improved forms of gas generator were described at the Bradford Meeting of the British Association by Mr. C. J. Woodward, B.Sc.:—Two forms of apparatus had been made. The first, shown in section in Fig. 15, and intended for large supplies of gas, consists of a circular stoneware vessel, A A, holding 3 or 4 gallons, and surmounted by a large cylindrical pipe, B. In the top of the vessel is a tubulure, C, to which is fitted a glass cylinder containing the granulated zinc or other gas-generating material. To the opening of the upper end of the glass cylinder is attached a cork and the delivery tube, D. A plug, E, can be raised or lowered by means of a cord, which, passing over pulleys, terminates in a ring, F. It will readily be seen in what way the apparatus is used. As seen in the figure, the plug, E, is immersed in the acid with which the stoneware jar is filled, and the liquid has risen by displacement into the glass cylindrical vessel, coming in contact with the gas-generating material, where of course the evolution of gas goes on. When it is desired to stop the flow of gas, the plug, E, is raised, the ring, F, being slipped over the stud, G. The acid now retreats from the glass cylinder, and the gas-generating material is left dry. At any time, then, when gas is wanted, it is only necessary to release the ring, F, the plug, E, falls into the acid, the zinc, marble, &c., becomes covered, and the flow of gas begins and can easily be arrested in the manner first described. The second form of apparatus, used when only a small supply of gas is wanted, consists of a Wolff's bottle, A, Fig. 16, to one tubulure of which is fitted a cork carrying a glass tube and piece of caoutchouc piping, B. This pipe, B, can be closed by a pinch-tap, C. To the other tubulure of the Wolff's bottle is fitted the adapter, D, in which is placed the zinc, marble, or other gas-generating material. To the upper end of the adapter is fitted a cork and tube, E, serving for the escape of gas, which is washed at F, and then passes on for use. To use the apparatus, the Wolff's bottle is charged with acid up to the level indicated in the figure. Then, on blowing air by the mouth by means of the tube, B, the pinch-tap, C, being open, acid is forced into the adapter, D, and gas at once comes off. The pinch-tap, C, is now closed, and the compressed air in the Wolff's bottle still keeps the acid in the adapter. When it is desired to stop the flow of gas, the pinch-tap, C, is opened, the compressed air escapes, and the acid in the adapter falls, leaving the gas-generating materials dry. Should the blowing of air by the mouth be deemed objectionable, an air-ball may be attached to the pipe, B. A safety-tube, C, is used, in order to prevent the liquid from the wash-bottle being drawn over on stopping the supply of gas.

FIG. 16.

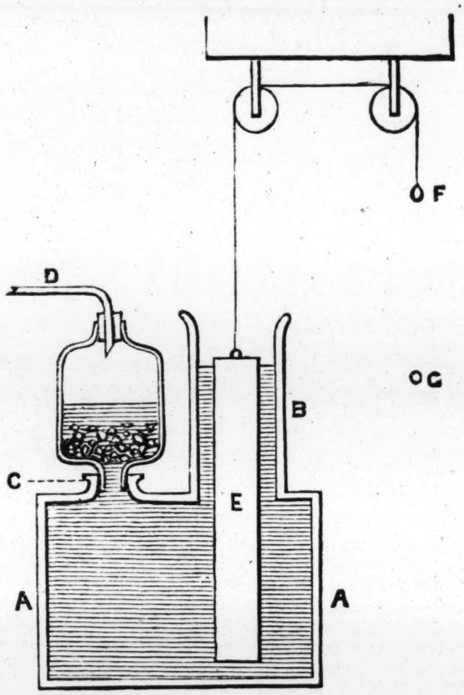
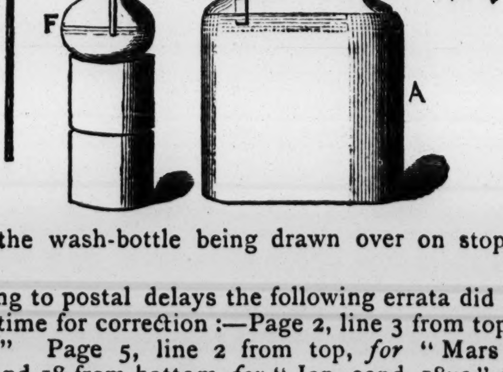
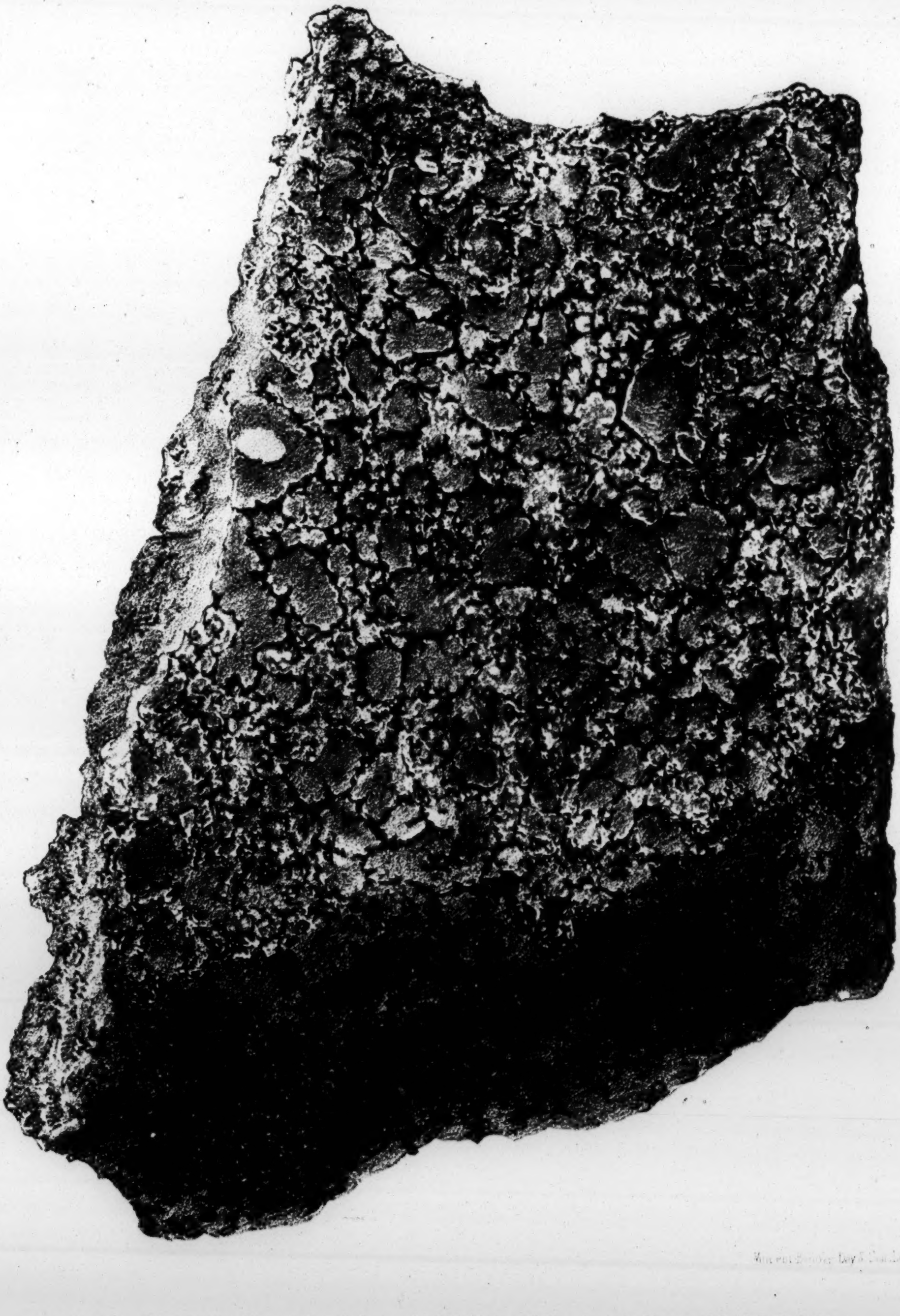


FIG. 17.



ERRATA.—Owing to postal delays the following errata did not arrive at the printing office in time for correction:—Page 2, line 3 from top, *for* "external" *read* "nocturnal." Page 5, line 2 from top, *for* "Mars" *read* "mass." Page 35, lines 2 and 18 from bottom, *for* "Jan. 22nd, 1872" *read* "Nov. 13th, 1871."

The Quarterly Journal of Science. *Nº XLII. April 1874.*



Vincent Brooks Day & Son Lith.

THE NATIVE COPPER MINES OF LAKE SUPERIOR.

By James Douglas

(See Page 173)